THE QUARTERLY JOURNAL OF
EXPERIMENTAL PSYCHOLOGY

Vol. II 1950 Part 3

PERCEPTUAL ANTICIPATION AND REACTION TIME

BY

E. C. POULTON

(*From the Medical Research Council Applied Psychology Research Unit, Cambridge*)

To determine the effect of perceptual anticipation upon reaction time, two different types of experiment were carried out. In the first a skilled response had occasionally to be altered at a given point after a variable warning period. In the second the subject had to react to two auditory signals separated by a short time interval which was systematically varied, the second signal being expected or unexpected.

It was found that lack of readiness to respond to a signal, as revealed by a lengthened reaction time, may be due either to the subject not having prepared himself, as he was not expecting the signal; or to the subject not being able to prepare himself in time. Preparation for reacting to the second of the two signals, when both are expected and have to be reacted to, never appears to take more than between 0·2 and 0·4 seconds, as judged by reaction time. On the majority of occasions it appears to be complete in 0·2 seconds. These times are shorter than those usually given, because the extra delay due to incorrect anticipation has been excluded. With intervals of 0·1 seconds or less, delay in the second reaction may be due to the mechanical difficulty of responding quickly enough, especially when the two reactions have to be made in opposite directions.

The finding that lack of readiness might be due to the subject not expecting the signal, and the further finding that preparation took longer when a skilled response had to be extended than when it had to be stopped, both suggest that so-called psychological refractoriness is due to lack of foreperiod in which to prepare for the response, rather than to a "psychological refractory phase" comparable to the refractory phase of nerves. If, by dividing his attention, the subject was able to prepare for his next response while making his previous one, so-called psychological refractoriness could be completely absent.

I

Introduction

Almost as soon as human performance was first studied in the psychological laboratory, it became apparent that the experimental subject's present response was not entirely a function of the present stimulus, but was co-determined by his past experience, previous learning, etc. However, little attention has as yet been given to the influence of the future on present performance. The experimental subject's performance can show two distinct kinds of anticipation. Receptor anticipation, such as is seen in eye-hand span, is a type of anticipation which is dependent basically upon the timing process. In a simple form of perceptual anticipation the subject is ready to respond in a certain way when he receives an expected signal, or actually responds in this way before he receives the signal, because he knows the signal is due, from his experience of the recent pattern of signals. In a more complex form there need be no external signal, the subject merely performing according to some aim, or idea of what is required of him.

The present paper describes a study of the effect of perceptual anticipation upon reaction time. When a number of signals, each of which had to be reacted to, were

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presented to the subject successively at irregular intervals of time (e.g. Telford, 1931; Vince, 1948a), it was found that the subject's reactions interacted with each other, the reaction time depending upon the time interval since the last reaction. This finding led Telford to the theory of a "psychological refractory phase," which he believed to be comparable to the refractory phase of isolated nerves. Attempts such as this to explain the complex in terms of the simple, although frequently successful in both physics and physiology, have on the whole proved singularly unsuccessful in psychology. It is becoming clear that the functioning of higher levels of psychological organization require new concepts specific to themselves, which cannot be arrived at merely from a consideration of the functioning of lower levels of organization. The human subject can anticipate, can make specific preparations for the immediate future, both on the sensory and on the motor side, in a way that an isolated nerve cannot.

In Telford's experiment no attempt was apparently made to eliminate the effect on the subject's performance of his expectancy when, after his last reaction, the next signal would arrive. Yet one would predict from Mowrer's work (1940) that the subject, not knowing whether the next signal would arrive after 0·5, 1·0, 2·0 or 4·0 seconds, would usually aim to be ready for the next signal somewhere between the middle two intervals of 1·0 and 2·0 seconds. In which case he would very definitely not be ready when a signal occurred after 0·5 seconds, and his readiness would have fallen off again if the signal was delayed for 4·0 seconds. The curve for predicted readiness would thus correspond closely to the curve for mean reaction times which Telford actually reported. It seems doubtful, therefore, whether his results have any further significance than this.

A similar criticism can be made of Vince's experiment. In addition, Vince presented some pairs of signals very close together, so that before the subject had had a chance to move his pencil upwards, he received a signal which indicated that he had to move it back again. In these circumstances the subject was not told definitely what he was expected to do, whether he was to move his pencil right up and then down again, or to curtail his upward movement as far as he could. (He was simply told to keep his pencil on the line he was following.) Where he did make the complete up and down movement, the very fact of having to make the upward movement before starting on the downward movement, may have delayed unduly the latter when the time interval between the two signals was short. The delays in reacting which Vince found thus appear to be relatively complex in origin. They cannot be accounted for adequately in terms of a single theoretical concept, and in fact she made no attempt to do so.

Although serial reaction times have been studied experimentally in this way, no attempt appears to have been made to study reaction times for the alteration of a complex graded response. Yet it seems possible that certain effects which are very important in everyday life, may be lost by experimental simplification to the level of serial reactions. Thus while one of the experiments described below is of the serial reaction type, but with expectancy controlled, the other uses complex graded responses instead of simple reactions. This is still a simplification of most everyday life situations, but at least it is less so.

II

Occasional Alteration of a Skilled Response at a Given Point

This experiment was designed to present the subject with a reaction time type of situation during the performance of a skilled task. For the setting of a skilled task,

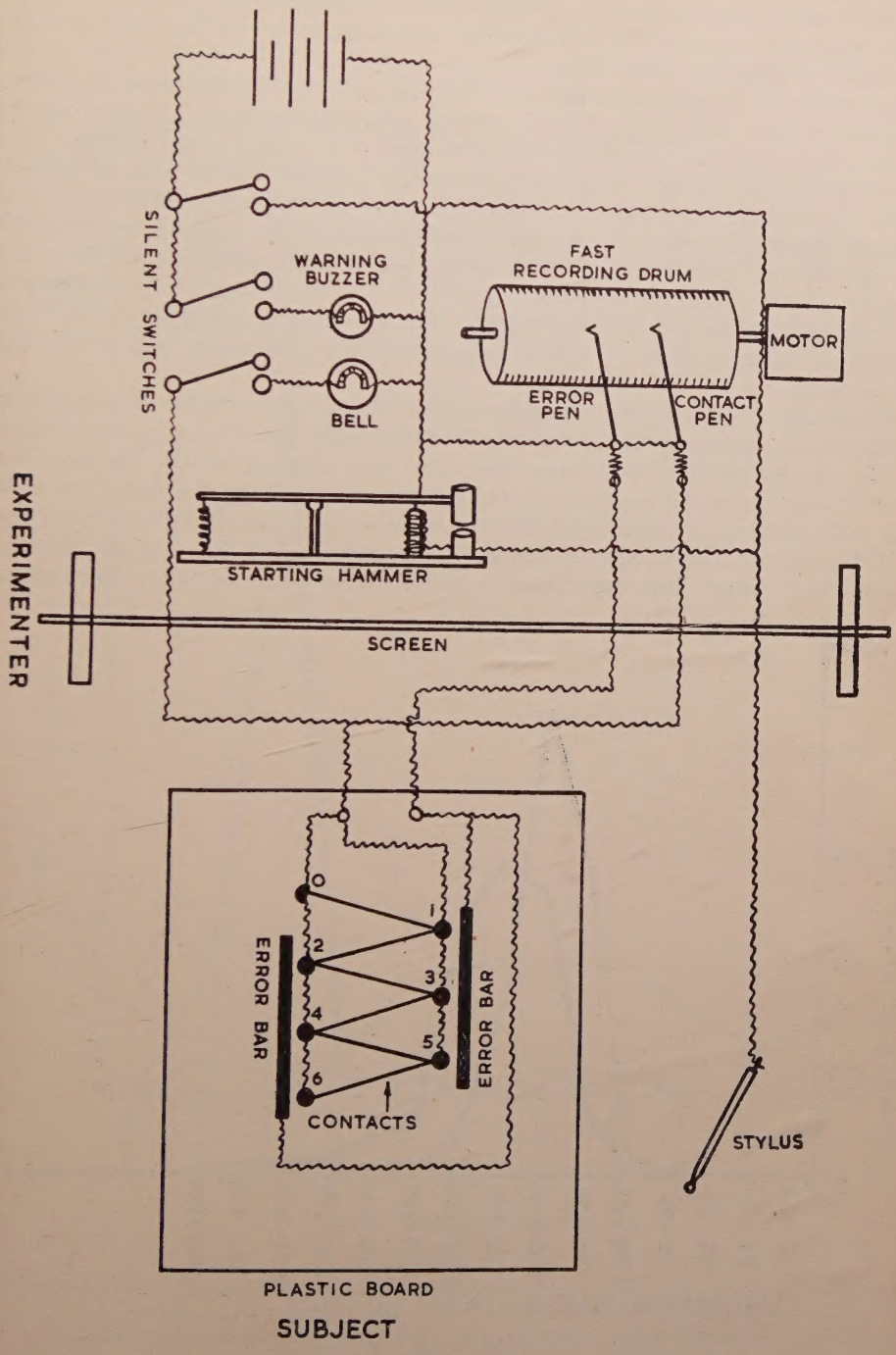

FIGURE 1

he general layout and electric circuits for occasional alteration of a skilled response at a given point.

rather than the classical reaction time experimental arrangement, is closer to situations arising in everyday life. The subject had to trace out a fully visible pattern as quickly as possible. (See Fig. 1.)

The pattern consisted of three identical V's lying on their sides one above the other. Electrical contacts were let flush into a plastic board at the bends, and just beyond these on either side was a brass bar which recorded errors due to overshooting. The subject was given a pencil stylus which he had to keep in contact with the plastic board. At the sound of a hammer, which followed a warning buzzer after an interval of approximately 2 seconds, he was instructed to move off the starting contact point (Contact O) and trace the pattern as quickly as possible, touching all the other electrical contacts (but not overshooting so as to touch either brass bar), trying to beat a target time he had just set himself. Full knowledge of results as to the time taken, and the errors made in over or undershooting at the bends, was given after each attempt.

On one day a bell sometimes rang during the subject's performance, which indicated that he had to stop on Contact 4 after tracing the first two V's, and omit the third V. On another day the subject was instructed only to trace out the first two V's; in which case, if the bell rang, he was to continue and trace out the third V as well. On a third control day the subject had to pay no attention to the bell when it rang, 12 of the subjects always having simply to trace out the first two V's, the other 12 all three V's. These three experimental procedures, and the lengths of the warning

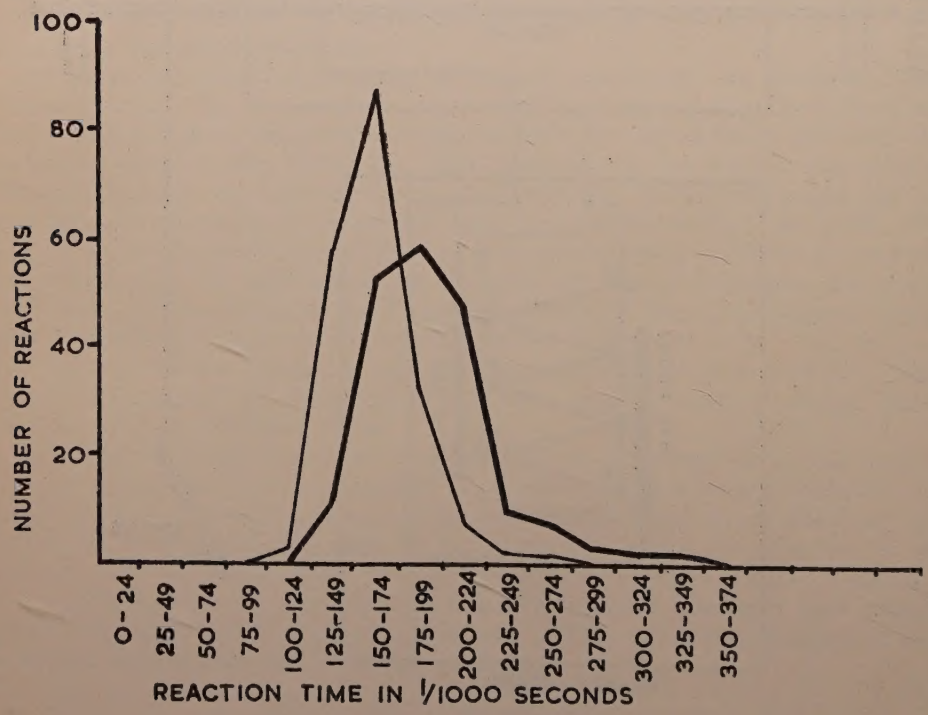

FIGURE 2

The distribution of simple and complex graded reaction times on the third day. Thin line: simple. Thick line: complex graded reactions. Chi squared = 92, compared with 16 at the 0·1% point.

period between the ringing of the bell and the subject reaching Contact 4 (the point at which the performance had to be altered on these occasions), were arranged in Latin Square form. The 24 subjects were all naval ratings, aged between 18 and 26 years.

(*a*) *Anticipation has a unifying effect in skilled performances.*

It was found that the mean reaction time at the start of the pattern tracing was about 0·025 seconds longer than the subject's simple reaction time when all he was instructed to do was to break the first contact with his stylus, the times lying in the region of 0·175 seconds for the auditory signal given. (See Fig. 2.) This is in line with earlier findings (Woodworth, 1938).

When the subject had suddenly and unexpectedly to change his performance, it was found that he needed a median of roughly about 0·25 seconds longer than an

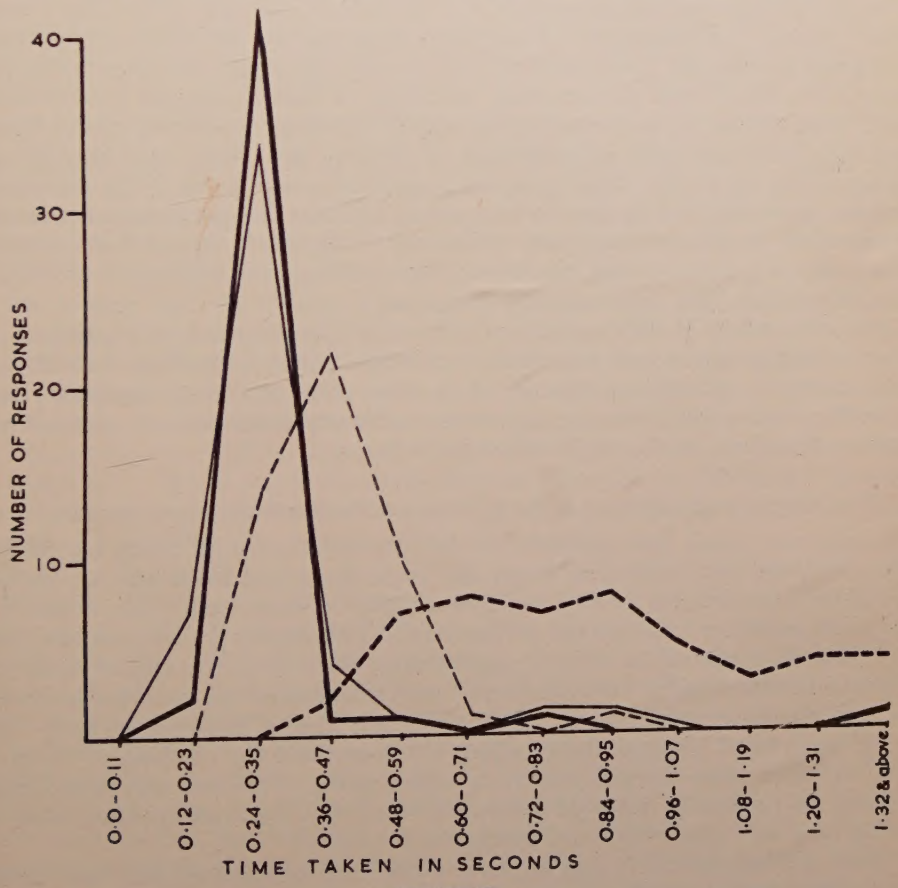

FIGURE 3

The distribution of combined reaction and movement times for the extension of a skilled performance. Thick dotted line: times for a sudden unexpected extension. Thin dotted line: a comparable sample of times from the start of the skilled performance, for a sudden expected movement of the same size. Thick line: times when a warning period of about 0·6 seconds was available during the performance. Thin line: a comparable sample of times when the warning was given before the performance started.

ordinary complex graded reaction time before he could stop the movement, and a median of about 0·35 seconds longer before he could start to extend it, there being significantly more long delays in the latter case. Further, the extension showed significantly more errors, from over or undershooting at the bends, than the rest of the performance, although such an increase in errors was confined to half the subjects. For the sudden unexpected extension, the distribution of times between the bell signal (which occurred as the subject touched what was normally the last contact, Contact 4), and the touching of the first additional contact by the subject, is shown in Fig. 3. Each time thus includes the subject's reaction time and his movement time.

This increase in the initial reaction time of the complex graded or skilled response, as compared with a simple response, and the apparent difficulty in suddenly and unexpectedly altering the skilled response, suggest that even such a simple skilled response as this is not merely a series of discrete responses. The whole performance is dominated by the anticipatory mental set or aim of the subject, and is to this extent a single whole. During the foreperiod the subject is preparing for the whole performance, not merely for the first little bit of it. At the starting signal the subject does not simply move off the first contact point, he begins to make a complex graded movement. And during the performance the subject is making a complex graded movement with a definite point of completion at the end, not merely moving from one contact to the next one. (This does not mean that every detail of the movement has been predetermined during the foreperiod, and that the performance is merely "triggered off" by the starting signal. The results in (*b*) below, suggest that secondary corrections are possible during the course of the performance, although there will be some delay before they can come into operation).

The performance of the experimental subject is thus not purely a function of the present stimulus and of past experience. It is also partly a function of the future, of the subject's anticipatory mental set or aim. This aim has a unifying effect, converting what might otherwise be a series of discrete responses to a succession of stimulus situations, into a single skilled performance.

(*b*) *Psychological refractoriness is due to the necessity of creating a new mental set.*

It was also found that provided the bell started to ring at about 0·6 seconds before the point was reached at which the performance had to be altered, reaction time when this point was reached could apparently be eliminated. This is shown in Fig. 3 for a sudden extension of the performance. Presumably the reaction time must have occurred concurrently with the performance of the task, the subject being able to divide his attention between his present performance and the intended alteration. The only occasions on which the performance was slowed down significantly often during the interval between the sounding of the bell and the reaching of the given point, occurred when the task had to be abbreviated. This was only with certain subjects, who generally reported afterwards that they had concentrated more on beating their set targets than on listening for the bell.

When the bell started to ring at about 0·3 seconds before the point was reached at which the performance had to be altered, the results were intermediate between no warning period and a 0·6 second warning period. Thus reaction time was lengthened in about 25 per cent. of the cases, which was only about a third as many as occurred when no warning period was available. Reaction time had apparently been eliminated in about 30 per cent. of cases. There were no significant reaction time differences between extending and abbreviating the movement with this length of warning period.

Practice obtained during the course of the experiment, and the subject's reported expectancy of the bell before starting to trace the pattern, appeared to bear little or no relationship to these findings.

Results such as these can be discussed either in terms of the subject's central nervous mechanism requiring some specific preparation before it can function, i.e. of a necessary foreperiod during which a specific mental set can be built up. Or they can be discussed in terms of some kind of "psychological refractory phase:" for example the recovery of neurosensory or neuromuscular systems, or the necessity of clearing the central nervous mechanism before it can function (Telford, 1931; Craik, 1947). If it is desired to retain any of these interpretations of a "psychological refractory phase," this phase must correspond to the relative refractory phase of nerves rather than to the absolute refractory phase, for the result above shows that it can be eliminated completely when the subject perceives and prepares to respond to a new item while continuing his present response. However, it is possible to take the view that such a division of attention requires a considerable effort on the part of the subject, and is thus comparable to the increase in the intensity of the stimulus which is required to make a nerve conduct during its relative refractory phase.

The conclusion that so-called psychological refractoriness can be eliminated completely for short periods of time when perception overlaps the preceding response, has also been reached from experiments in which the subject was presented with display items successively at high speed. Under these conditions the subject's speed of response was sometimes such that there was no evidence of so-called psychological refractoriness. On such occasions there was sometimes definite evidence that the subject was perceiving one display item while responding to a previous one (Poulton, 1950).

The finding that the reaction time was in general longer when the performance had suddenly and unexpectedly to be extended than when it had to be stopped, suggests that the length of the reaction time was partly a function of the future, of what the subject had to do at the end of it. So-called psychological refractoriness can therefore contain a time element corresponding to the preparation for the succeeding task, such as normally occurs in the foreperiod of the classical reaction time type of experiment. Thus if any of the above interpretations of a "psychological refractory phase" is retained, two theoretical concepts appear to be required, whereas the single concept of a necessary foreperiod is sufficient alone.

But in any case this analogy to the refractory phase of nerves is misleading. For it implicitly assumes that the classical reaction time, a product of an artificial laboratory situation, is the normal state of affairs in the absence of an immediately preceding reaction. But the human subject is never ready to respond as quickly as this, except after a foreperiod; while a healthy nerve is always ready for almost instantaneous response, except just after it has been stimulated. In the classical reaction time type of experiment, the preceding foreperiod leads to the reduction of reaction time to a minimum. It is when no time is available for such a foreperiod that the reaction time is lengthened, and psychological refractoriness is said to occur.

On this view the "psychological refractory phase," a theoretical concept resulting from an approach to human performance which seeks to interpret it purely in terms of past and present, needs to be broadened to take the future into account. The subject delays because he is not ready; but his lack of readiness is due, not to his having to recover from what he has just done, but to his having to prepare himself for what he has to do next, having to create a new mental set. As Bartlett (1947) has suggested, before a task can be performed, mental as well as physical posture has to be appropriate. If this preparation can occur while a previous response is being made, no so-called psychological refractoriness need be apparent.

III

Reactions to Two Successive Signals

This experiment was designed specifically to discover whether so-called psychological refractoriness was due purely to the subject not being ready for a response because he was not expecting to have to make it, and so had not prepared himself; or whether under certain conditions refractoriness was at least partly due to the subject not being able to prepare himself. For the results of the experiment in §II above (Occasional Alteration of a Skilled Response at a Given Point) suggested that provided the subject had plenty of time in which to prepare himself, and knew when he had to respond (or change his response), he might be able to do it without any so-called psychological refractoriness, regardless of what he had to do immediately beforehand during the foreperiod.

In this simplified version the subject had merely to respond to two auditory signals, a bell and a hammer, the second signal following the first after a short interval of time, by moving a key a short distance in one direction, and then in the reverse direction. The pairs of signals occurred in series of 12, at intervals of 1·7 seconds. In each series the first signal was omitted once, and the second twice, in order to prevent premature reactions. The time interval between the two signals in each pair was reduced from 0·8 seconds in the first series, to 0·1 seconds in the fifth. In each series the subject knew when to expect the signals. On the first day the subject was instructed to respond as quickly as possible to both signals. On a second day, in one part he was instructed to pay particular attention to making his reaction to the second signal as quick as possible; in another part the second signal only occurred twice in each series, and the subject was instructed to try to make his reaction to the first signal as quick as possible. The two parts, two signals, and two response directions were arranged in Latin Square form. After each series, the subject was given knowledge of any false, very early, and very delayed reactions which he had made. The eight subjects were all naval ratings, aged between 19 and 26 years, none of whom had performed in the previous experiment.

The subjects reported that they found the experiment difficult to perform as instructed, one subject actually breaking down and refusing to go on for a while. The difficulty appeared to be associated with the catch episodes. For the absence of the first signal once in each series changed the task from being of simple reaction to being of 2-choice reaction type, the subject having to decide in this case whether the signal was the usual first or second signal, and then to react accordingly. Yet he was instructed to react in general as quickly as possible, which he apparently took as indicating that he was to treat the task as being of simple reaction type.

(*a*) *Psychological refractoriness can result from disturbances during the foreperiod.*

It was found that when the practised subject knew that the second signal would follow the first at an accustomed interval of 0·2 seconds (except for the customary checks to prevent false reactions), and was instructed to concentrate on keeping his reaction times to the second signal as short as possible, the distribution of these reaction times was significantly different by chi squared from the distribution of his reaction times to the first signal. With all three different sets of instructions, and with all the different time intervals between signals which were used, the distribution of reaction times to the first signal did not differ significantly from the distribution of reaction times to an expected single signal. The distributions for an expected second signal after 0.2 seconds, and for an expected single signal, are shown in Fig. 4.

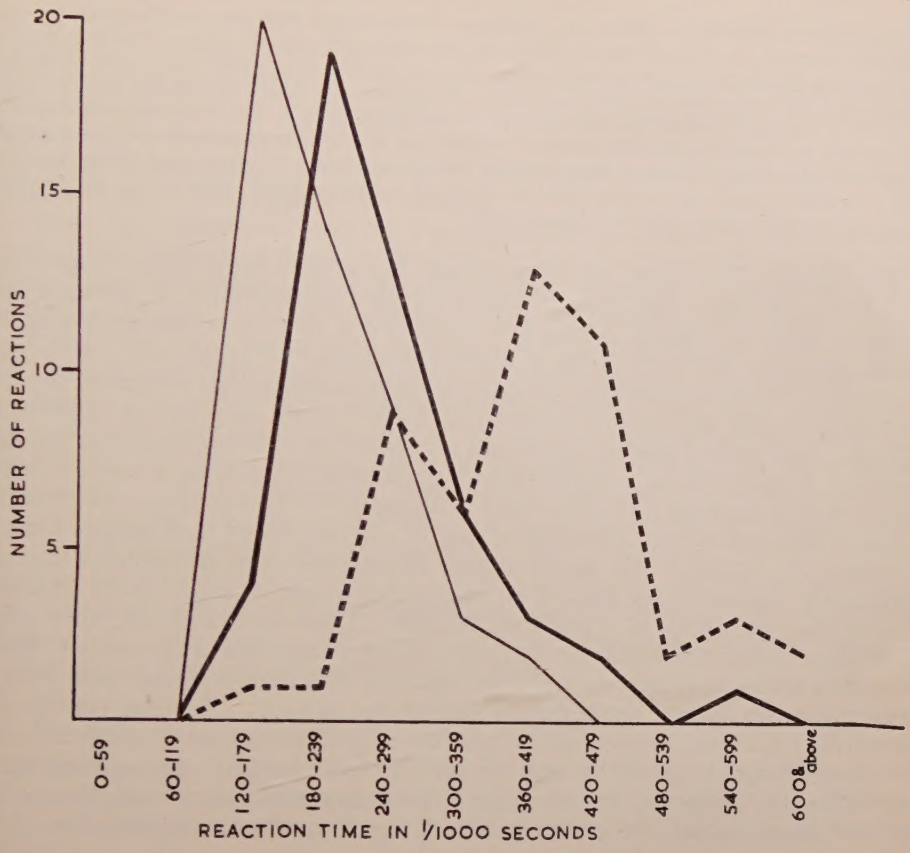

FIGURE 4

Distribution of reaction times to auditory signals on the second day (six reactions from each of eight subjects). Thin line: reaction times to a single signal. Thick line: to a second signal 0·2 seconds after the first, the second signal being expected. Dotted line: to an unexpected second signal.

With this 0·2-second interval between signals there were eight false reactions, all occurring during the 24 catch episodes when one or other signal was omitted. Four of these false reactions occurred on the eight occasions when the first signal was omitted, the subject in each case responding after the second signal as he normally did after the first. He apparently treated the task as being of simple reaction type, responding as soon as he heard a signal, instead of treating it as being of complex 2-choice reaction type, with two different responses according to the nature of the signal. This is confirmed by the distribution of reaction times to the first signal, which does not differ significantly from the distribution of reaction times to a single signal. It was certainly not intended that the catch episodes should alter the nature of the task to being of complex 2-choice reaction type in this way. The surprising finding is that in four out of these eight catch episodes the subjects were able to inhibit their normal response to the first signal. The other four false reactions occurred after the soft click which replaced the usual second signal in 16 of the catch episodes, the reaction times as measured from this click varying from 0·17 to 0·58 seconds. In these cases either the subject was responding at an accustomed interval after the first signal, without necessarily paying any attention at all to the second signal; or he was responding to the soft click, having failed to distinguish between it and the normal second signal. However, in no case in the 96 episodes with his accustomed time interval of 0·2 seconds between signals, did the subject react noticeably too early to the second signal, as might have occurred if he had neglected to

attend to it altogether. Assuming the same incidence in the non-catch episodes, it is probable that in 12 ± 6 of the reactions to an expected second signal, the distribution of the times of which are given in Figure 4, the signal did not receive adequate attention, as judged by the number of false reactions. Whether this was because the subject could not give it adequate attention and also react as quickly as he did, or simply because he did not do so, it is impossible to tell. At all the other time intervals between signals there were less false reactions than at this 0·2-second time interval; and false reactions were rare both when the subject had to react to a single signal, and in the series when the second signal only occurred occasionally.

The results above show that even though the subject knew when to make his second reaction, and could thus in theory prepare himself for it in advance, yet having to react to a signal during this foreperiod a short time before making his second reaction, did delay it on a significant number of occasions. Apparently reactions to two expected signals, either of which on occasions may not occur, requires not a single mental set, but two distinct sets, both of which need time in which to be built up. And part at least of the second set cannot be built up until after the first reaction. A foreperiod of a certain length, undisturbed by having to react to other signals, is thus required if reaction time to a signal is to be minimised consistently.

When the subject was instructed to neglect the first signal, and simply to react as quickly as possible to the second signal, the reaction time distribution was not significantly different from that of an unexpected single signal, both for intervals between the two signals of 0·4 and 0·1 seconds. There were, however, a few more rather long reaction times with the distracting signal, mainly due to one subject. There were also a few more premature and false reactions with the 0·1 second time interval between the two signals, again mainly due to one subject. This was presumably because the 0·1 second interval encouraged the subject to react at the expected time of the second signal, instead of waiting until he had heard it. An expected signal itself during the foreperiod therefore has little if any effect on the reaction to an expected second signal. It is only a second signal to which a reaction has to be made which has any significant disturbing effect.

It will be seen from Fig. 4 that, as judged by the length of the reaction times, the subject was frequently as ready for his second reaction as he was for a single reaction, although subjectively this seemed to require a good deal of effort. This suggests that the kind of so-called psychological refractoriness which cannot be eliminated by forewarning the subject, is only true statistically; that although it holds for a group of responses as a whole, yet it need not occur in every individual case. Readiness as revealed by reaction times, seems to vary from instant to instant. All but the extreme degrees can apparently be achieved relatively quickly, provided the subject knows that it is required of him, and is prepared to make the necessary effort. Simply presenting mean reaction times fails to call attention to this distributional overlap. It also fails to show the skewness of the reaction time distributions, which may invalidate the use of the "t" test in assessing the significance of the differences between them.

When the interval between the two signals was reduced to 0·1 seconds, the delay in the second reaction was much more marked, there being relatively little overlap between the two distributions. But it was found that the distribution of time intervals between the two reactions was not significantly different from the distribution of time intervals which occurred when the subject was instructed to make the double response to a single signal, in spite of there being a few more of the rather longer time intervals between the two discrete reactions. Hence at least part of the delay in the second reaction may have been due to the mechanical difficulty of

responding quickly enough, and not to so-called psychological refractoriness. This is a defect in experimental technique which must almost inevitably occur when the second reaction is the opposite of the first. With such an experimental arrangement measurements of so-called psychological refractoriness must be suspected whenever the two signals are separated by an interval of about 0·1 seconds or less. This defect can at least partly be overcome by making the second reaction an extension of the first (e.g. Vince, 1948b; Hick, 1949).

When the interval between the two signals was increased to 0·4 seconds, the distributions of reaction times to the first and second signal were not significantly different, provided the second signal was expected. Apparently 0·4 seconds gives the subject ample time in which to complete his preparations for his second response, provided he knows he has to do so, and makes the necessary effort. This elimination of the additional delay when an interval of 0·4 seconds was available between the two signals (the second signal being expected), is comparable to the elimination of the additional delay (and apparently of the reaction time also) at the given point ahead in §II (*a*) when a warning period of 0·6 seconds was available during the performance of the skilled movement. Preparing for a simple choice reaction (to respond or not) at a given time ahead, took about as long as preparing for a definite change of a skilled response at a given point ahead while performing. In both cases the intended response could only receive part of the attention, the rest of the attention being taken up in the first case in determining whether or not the second signal had occurred, for there were occasional checks to prevent false reactions; and in the second case in performing the skilled movement.

(*b*) *Psychological refractoriness can result from incorrect perceptual anticipation.*

When the second signal only followed the first relatively infrequently, and the subject was instructed to concentrate on keeping his reaction times to the first signal as short as possible, the distribution of his reaction times to the second signal showed relatively little overlap with the first at any of the time intervals between 0·1 and 0·85 seconds. The combined distributions for intervals between signals of 0·1, 0·2 and 0·4 seconds is shown in Fig. 4 above. There was little difference between the reaction time distributions at any of these time intervals.

Fig. 4 shows the considerable lengthening of the reaction time to an unexpected second signal, as compared to an expected second signal when the subject was instructed to react to it as quickly as possible. This and the finding that the reaction time to an unexpected second signal was relatively independent of the time interval between the two signals, suggests that the delay was due to the subject not expecting the second signal when it occurred, and so not having prepared himself for it. It was not due simply to the subject not having had time for such preparation. Fig. 4 thus illustrates two distinct factors in so-called psychological refractoriness: lack of readiness due to insufficient time in which to get ready, and lack of readiness due to not realising that there was anything to get ready for. It will be seen that the latter can cause far more delay than the former.

Previous experimenters on the "psychological refractory phase," other than Hick (1948), do not appear to have made this distinction. As usually designed, the subject receives single signals at irregular intervals, with an occasional pair of signals close together. He may not be told what to do if a second signal demands a movement in the opposite direction to the first before he has had time to make the first movement. Sometimes he is not even warned that such pairs of signals are liable to occur from time to time. In such experiments different subjects may be trying to do

different things when a second signal does closely follow the previous one. And it is left to the reader to infer from the experimental design and instructions how far the so-called psychological refractoriness which is reported is due to insufficient time in which to get ready, and how far it is due to the subject not getting ready as he is not expecting a second signal so soon. For example, Vince's conclusion (1948a) that: "A second stimulus is not likely to evoke a second response until an interval of about 0·5 seconds after the appearance of the first stimulus has elapsed," must be restricted to the experimental situation from which it was deduced. As described in paragraph 3 (*a*) above, when the subject is expecting a second signal, he can always be ready 0·4 seconds after a previous signal to which he has responded. Often he can be ready after 0·2 seconds. Long delays are due predominantly to incorrect anticipation, and not to actual inability to get ready in time.

This finding again demonstrates the inadequacy of discussing so-called psychological refractoriness in terms of a "psychological refractory phase" analogous to the refractory phase of nerves. However long a time the subject is allowed in which to recover from his previous reaction, if he is not expecting to have to make a further reaction, he is unlikely to prepare for one. When a signal further does arrive, therefore, his reaction to it will be delayed.

This additional delay which occurred when the second signal was not anticipated, is comparable to the additional delay which occurred in §II (*a*) above, when the skilled movement suddenly had to be altered. That the delay was frequently much longer in the case of the sudden alteration of the skilled movement, may have been because here a definite anticipatory mental set had to be discarded, as well as a new mental set adopted; whereas in the case of the unexpected second signal there was no definite mental set to be discarded.

(*c*) *The effect of simplifying the experimental situation.*

Studying the effect of perceptual anticipation upon reaction time with the simplified experimental arrangement above, which is the only way as yet in which the so-called "psychological refractory phase" appears to have been studied directly, would not have suggested that the psychological refractoriness which occurs even when the subject knows what to expect, is due to his having to prepare himself for his future response, rather than to his having to recover from his previous one. For there could not have been two different responses to make in different parts of the experiment, one requiring a longer preparatory foreperiod than the other, as was found when the skilled response had either to be extended or stopped. This may be a further reason why so-called psychological refractoriness has up until now been treated as a function of the past, rather than as a function of the future; why it has been discussed in terms of a "psychological refractory phase," rather than in terms of a necessary foreperiod.

The findings of these two comparable experimental arrangements thus illustrate Bartlett's (1932) contention that although an experimental simplification may be useful in revealing effects which operate in a complex situation, yet that some effects may be lost by such simplification. One solution of this dilemma would appear to be to experiment as here at the level of both simple and more complex behaviour.

IV

General Discussion and Conclusions

So-called psychological refractoriness contains two distinct elements, which do not seem to have been clearly distinguished by most previous experimenters. Firstly

the subject may not be ready to respond because he is not expecting a signal, and so has not prepared himself. It is this element which seems to cause the long delays before reacting, especially when the subject is expecting to have to complete the skilled task he is performing. And secondly the subject may not be ready because he has not had time in which to prepare himself. Thus if he has had to respond to an expected previous signal 0·2 seconds before the critical signal, his readiness as shown by his reaction time will be less complete on a significant number of occasions than when he has no such previous signal to which to respond. Even in this case, however, he will seem to be just as ready on the majority of occasions. If the subject is allowed 0·4 seconds in which to prepare himself, he can always be ready. These times are shorter than those usually given, because the extra delay due to incorrect anticipation has been excluded. The mere occurrence of an expected signal just before the critical signal has little or no effect when the subject is told to neglect it. With intervals of 0·1 seconds or less between two signals, to both of which a response has to be made, at least part of the delay in responding to the second signal may be due to the mechanical difficulty of responding quickly enough, such as must almost inevitably occur when the second response has to be made in a direction opposite to the first.

Leaving both kinds of so-called psychological refractoriness unseparated, and discussing the combination under the heading of a "psychological refractory phase," is misleading. Firstly, the lack of readiness which is due to a failure on the part of the subject to prepare himself, because he is not expecting to have to respond, is evidently not due to his having to recover from his previous response. And secondly, the time required for such preparation was found to be longer when a skilled response had to be extended, than when it had to be stopped. This suggests that at least part of this time was occupied in the subject preparing for what he had to do next, and not in recovering from what he had just done. It will thus be seen that the analogy to the refractory phase of nerve is misleading. For whereas a healthy nerve is always ready for almost instantaneous response, except just after it has been stimulated, in the intact human subject it is the presence of an immediately preceding foreperiod which reduces reaction time to a minimum. Without such a foreperiod during which the subject deliberately prepares himself, the necessary minimum duration of the foreperiod varying with the task to be performed, reaction time will be lengthened, even in the absence of an immediately preceding reaction. Further, if by dividing his attention the subject is able to prepare for his next response while making his previous one, so-called psychological refractoriness may be completely absent.

The performance of a skilled response is dominated by the anticipatory mental set or aim of the subject. The initial reaction time is longer than a simple reaction time; if the performance has suddenly and unexpectedly to be changed, there is frequently a very long delay before this can occur, much longer than the delay in reacting to a signal which unexpectedly follows the reaction to a previous signal; and if the change in the skilled response involves an extension of the movement, this extension is likely to be erroneous, particularly in the case of certain subjects. The subject's anticipatory mental set or aim thus has a unifying effect, converting what might otherwise be a series of discrete responses to a succession of stimulus situations, into a single skilled performance.

The approach to human performance which attempts to interpret it purely in terms of past and present, thus needs to be broadened to take the subject's mental set towards the future into account. The concept of a "psychological refractory phase" comparable to the refractory phase of nerve, is an example of this backward

orientation on the part of psychologists. Attempts to split up a skilled performance into elements, each of which is believed to depend purely on the elements immediately preceding it, is a further example. The evidence here presented shows that in neither case is this the complete picture. Present performance is always dominated by the subject's idea of the immediate future, by what he expects to happen, and by what he is trying to accomplish.

A comparison of the findings of the two different experimental arrangements used, showed that only the more complex experimental arrangement was able to suggest that the so-called psychological refractoriness which occurs even when the subject knows what to expect, is due to his having to prepare himself for his future response, rather than to his having to recover from his previous one. Although an experimental simplification may be useful in revealing effects which operate in a complex situation, yet some effects may be lost by such simplification.

It is recommended that in any further experiments on so-called psychological refractoriness, the experimental design should ensure that:—

(i) The subject's expectancy of the signal should be controlled as far as is possible.

(ii) He should receive definite instructions as to what he is to do when two signals closely follow each other.

(iii) When intervals of 0·1 seconds or less between the two signals are studied, the required response to the second signal should be in the same direction as the response to the first signal, as in experiments by Vince (1948b) and Hick (1949).

I am greatly indebted to Professor Sir Frederic Bartlett, F.R.S. for presenting the problems and theoretical background out of which these experiments have developed. Also to Dr. N. H. Mackworth, Dr. W. E. Hick, Miss M. A. Vince, and Mr. H. Kay for their help. The experimental subjects were supplied by the Royal Navy. These experiments were carried out while in receipt of a grant from the Medical Research Council.

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(*Received 1st May*, 1950.)

FIGURAL AFTER-EFFECTS IN STROBOSCOPIC MOTION

BY

L. WEISKRANTZ*

(from the Institute of Experimental Psychology, Oxford)

The present investigation is designed to determine whether figural after-effects of the type reported by Köhler and Wallach occur in stroboscopic motion following prolonged visual fixation of a stationary pattern. The lengths of the paths of two simultaneously presented spots in stroboscopic motion were adjusted so that they appeared equal to each of the 21 subjects. Each subject then fixated a satiation pattern for two minutes, after which he reported whether the paths, unchanged by the experimenter, were equal or not, and in which direction he observed a distortion, if any. The satiation pattern was so designed that a prediction of a distortion in one direction could be made in the subject's first judgment, while a slight change in the arrangement would yield a prediction of a distortion in just the opposite direction in the second judgment. The results are taken to support the finding Köhler and Wallach that the contours of a test pattern recede from the contours of the satiation pattern, even when the test pattern is a stroboscopic one.

I

Introduction

The term "figural after-effects" has been used by Köhler and Wallach (1944) to refer to distortions of perceived form which occur subsequent to prolonged stimulation. If, for example, a point on a given pattern (referred to here as the satiation pattern) is visually fixated for a period of about one to two minutes, a pattern (the test pattern) presented immediately afterwards will be found to be distorted in the following manner: (*a*) contours of the test pattern will tend to recede from the position where the contours of the satiation pattern lay; (*b*) the amount of this recession, up to a certain limit, will vary directly with the distance from the position of the satiation pattern. The latter relationship is termed "the distance paradox" by the authors. The purpose of the present investigation is to determine whether such prolonged stimulation, or "satiation," will introduce similar distortions into the subsequent perception of stroboscopic motion. The experiment reported here tests only the recession principle (*a*), and not "the distance paradox."

II

Method and Apparatus

The method was the comparison of perceived length for two simultaneously presented paths of apparent movement, the comparisons being made before and after satiation. The satiation pattern was so designed that in one case one of the paths should be distorted as so to render it longer than the other, and in another case, just the opposite, i.e. shorter than the other path. The arrangement used was the following:

Conditions were so arranged that T_1 and T_3 were presented simultaneously for a short time followed quickly by the simultaneous presentation of T_2 and T_4 (Fig. 1). Thus stroboscopic motion was obtained so that there appeared to be two spots, one above the other, moving rapidly from left to right. The sequence was repeated a

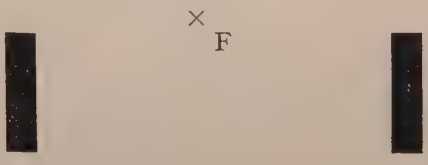
* Working with a Catherwood Foundation Fellowship.

sufficient number of times to meet the requirements of this experiment. The room was dimly illuminated so that the subject could see the fixation point F. While

FIGURE 1

fixating this point, the subject was instructed to report whether the distance through which the top spot moved was larger, smaller, or equal to the distance through which the bottom spot moved. The distances were varied following each report so that they appeared phenomenally *equal* to the subject. After this had been accomplished, the lights were turned on and the following satiation pattern was presented, the subject being instructed to fixate the point F for 2 minutes (Fig. 2). Following this,

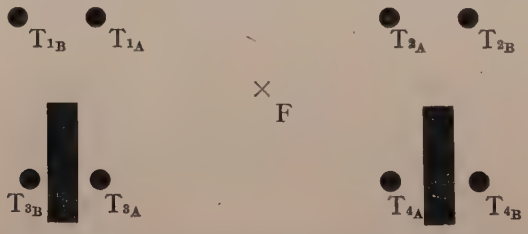
FIGURE 2



the subject once more compared the two distances of the moving spots, although the objective relationships had not been altered by the experimenter.

It will be seen (Fig. 3) that if the spots are arranged so that the distance between

FIGURE 3



them is just shorter than that between the two black bars, and if figural after-effects manifest themselves, the bottom spots should recede slightly from the area of the bars, and hence in the subsequent stroboscopic motion, the bottom path should appear shorter than the top. We shall call this condition A. On the other hand, if the distance between the spots is just larger than that between the bars (condition B), the spots should now recede in the opposite direction, thus making the bottom path longer than the top. Both conditions A and B were presented to all subjects, in some cases with A first, followed by B, and in others with B first. Fixation of the satiation pattern was introduced for both conditions.

The following procedural details should be noted:

1. It was found easier to induce perception of motion if a subject was instructed that he was going to see "two spots moving from left to right across the screen," rather than if the objective stroboscopic relationships were explained to him. In

preliminary experiments, when the subject was aware of this actual arrangement, he often reported that it was very difficult to see motion, but such a difficulty was only rarely present with the above instructions.

2. Fixation of the satiation pattern was monocular, the subject holding a black card over one eye. The test judgment was made with the other eye. In preliminary experiments, it was found that the very vivid negative after-images which appeared after fixation often seriously distracted the subject while making the test judgment. The present procedure reduces the amount of interference, although it also probably diminishes the strength of the effect being sought.

3. It was permissible, of course, only in the first of the two conditions to adjust the distances between the spots so that the paths were phenomenally equal to the subject; such an adjustment in the second condition would counteract the effect being sought. Thus, in the second condition, the spots were arranged so that the distances T_1T_2 and T_3T_4 were objectively equal. The objective distances which satisfied phenomenal equality in the first condition were measured for each subject.

4. While the subject was fixating the satiation pattern, the experimenter made slight irrelevant noises in the general vicinity of the lanterns to make it appear feasible to the subject that the apparatus was being altered. It was discovered in the preliminary investigations that if this was not done, the subject usually expected the two paths to remain phenomenally equal in the test case and, in some cases, even if differences appeared, would refuse to be so obviously "tricked" by the experiment. The subject's attention was further directed to the possibility of change by asking him, while he fixated, "not to mind any noise" behind him.

5. If the subject reported in the test judgment that one path was longer than the other, he was requested to indicate the extent of distortion by selecting the appropriate item in a series of 10 drawings in which the distortion grew progressively greater in steps of 0·5 in., or a step of 2·9 per cent. change in the case of A, and 2 per cent. in the case of B.

The timing mechanism employed to obtain stroboscopic motion was a motor-driven rotating cam closing and opening contacts of two lanterns in the appropriate fashion. The speed of rotation could be varied by means of a rheostat. Generally the time interval between successive presentations was about 0·05 seconds, each lantern remaining lit for approximately 0·18 seconds. The lanterns were approximately 9 ft. away form the screen; the subject sat about 7 ft. away from the screen. In condition A the distance between T_3 and T_4 was close to 7 in. (yielding a visual angle of 4° 46′); in B this distance was close to 10·5 in. (visual angle, 7° 8′), the spots falling 0·5 in. outside the contour of the bars in both cases. The distances T_1T_3 and T_2T_4 in all cases were 4·2 in. (visual angle, 2° 52′). The black bars were 3·3 × 0·75 in. (visual angles, × 2° 14′ × 0° 30′).

Twenty-one subjects were used in the experiment, most of them being Oxford students. All of the subjects were unaware of the purpose of the investigation, but it was found that many of these students, at least, were fairly suspicious of the nature of the experiment and it was rather difficult to obtain subjects who would adopt a naïve, phenomenonological attitude.

III

Results

The results are recorded in Table I. By a + result is meant one which satisfied the predicted change, while a — result was one which was distorted in a direction

opposite to prediction. Zero indicates that no difference between the two distances was reported.

TABLE I

	+	−	O	P ≷
Combined Results (A and B)	22	3	17	0·000
Average reported distortion ..	5·9%	4·8%		
Condition A	12	1	8	0·002
Average reported distortion ..	7·1%	5·7%		
Condition B	10	2	9	0·019
Average reported distortion ..	4·8%	4·0%		
First reading (12 A, 9 B)	13	1	7	0·001
Average reported distortion ..	6·9%	5·7%		
Second reading (9 A, 12 B)	9	2	10	0·032
Average reported distortion ..	4·8%	4·0%		

It will be noted (Table I) that the differences between positive and negative results are large, and that the probability that such a discrepancy between plus and minus is a chance result is less than 5 per cent. in all cases. The average reported distortion also is greater with the positive results than with the negative, although the difference is probably not significant. A chi-squared test employed to test the hypothesis of chance relationship between conditions A and B yields a chi-square of 11·58, which is well above the requirement for 1 per cent. significance with 1 degree of freedom.

It should be remarked, also, that with no subject was a double negative result obtained, although a double positive result was obtained seven times. In fact, never was there a negative result in which there was not also a positive result obtained for the other condition.

Strictly speaking, it is not crucial that the results be significant for the second reading as well as the first, since there are undoubtedly additional factors of a perseverative nature being introduced when the first judgment is made. It will be noted that the results for the first reading are more favourable than for the second.

In the calculation of probabilities and in the chi-square test, the zero results were ignored. The justifications for such a procedure are as follows:

1. Other figural after-effects experiments have noted considerable individual differences, particularly as regards the size of the distortions. Also, since subjects were asked to describe changes in shading, position, and shape which occurred during the fixation period, it was possible for the experimenter to predict in advance many zero results in those cases where the subjects did not report the characteristic changes during fixation (Köhler and Wallach, *op. cit.*).

2. The resistance which many subjects exhibited towards adopting a naïve phenomenological attitude would most certainly tend to increase the number of zero results. One subject, for example, some days after the experiment confessed that she was not going to be "tricked into saying one path was longer than the other." In addition, it is suspected that the loud clicks emitted by the timing mechanism tended to make subjects suspect that the two distances were objectively equal. More satisfactory apparatus is now being constructed.

3. Some subjects expressed difficulty in making the required comparisons between the paths of motion. It is reasonable to suppose that in such cases the results would most likely be zero.

Finally, it is necessary to establish that the distortions which were reported, could not have been the result of the actual objective discrepancies which were introduced when the subject reported phenomenal equality at the very first stage. Even if these discrepancies were relatively large and in the same direction as the reported distortions, it could still be argued, of course, that the subject nevertheless did judge them to be equal initially and that the distortions are most correctly to be compared with these initial settings rather than with objective dimensions. In order to avoid debate, however, it would be well to consider these discrepancies briefly. Also, it was unavoidable in making the new settings for the second readings to make small errors of adjustment, but in only three instances were these errors greater than 0·1 in. (a 1·4 per cent. error in condition A and a 0·95 per cent. in condition B). These three errors are included in the present considerations.

Considering only those cases in which positive results were obtained, we find that out of 22 cases, in 13 the two distances were objectively equal (to the nearest 1 in.). Of the remaining nine cases, two of the intial discrepancies were in the opposite direction to the reported distortion and hence need not be considered here. The average objective discrepancy for the remaining seven cases was 1·87 per cent., while the average reported distortion was 5·04 per cent. If a t-test for paired comparisons is applied, the resulting t of 3·96 is significant at the 1 per cent. level, for 6 degrees of freedom. Hence, a hypothesis to the effect that the reported distortions were chance errors in judgment due simply to the objective discrepancies is not tenable. Even if it were tenable, it would affect only seven cases out of the 22, at most (and would also eliminate one of the negative cases). Finally, since the assumptions which such a hypothesis would rest upon are confronted with the observation that the subjects nevertheless did judge these objective distances to be phenomenally equal, it is not felt that it need be taken as presenting a serious challenge to the present interpretation of the results.

IV

Discussion

The present investigation extends the observations of figural after-effects to the case of stroboscopic motion and supports the finding of Köhler and Wallach that the contours of a test pattern recede from the contours of the satiation pattern.

In considering the present experimental results, one might question whether the effect of satiation was, in fact, to alter the path of stroboscopic motion directly, or whether the lower spots T_3 and T_4 were simply altered in the predicted fashion, and the subject was simply comparing the two distances between the two sets of dots. Such a question, of course, rests wholly upon a consideration of whether the subject did or did not, in fact, perceive two dots in simultaneous motion, or whether he merely saw four dots. That in many cases, at least, in which positive results were obtained, the perception of motion was genuine is supported by the fact that many of these subjects were very surprised when informed that there was actually no objective motion taking place. In one case, this disbelief was so strong that the subject was not fully convinced until the experimenter lengthened the time interval considerably and allowed the subject to examine the apparatus carefully. It is fair to assume, on the basis of such observations, that the figural after-effects were directly present in the phenomenal motion and that the experiment is not simply a repetition of a case already fully investigated.

As has already been mentioned, this experiment does not test "the distance paradox," and in that sense is only preliminary. Indeed, it is suggested that any assumption that figural after-effects always present themselves in the same general manner in stroboscopic motion as in the case of other perceptual phenomena already investigated, should be carefully guarded against. Preliminary investigations with other satiation patterns throw some doubt on such an assumption, although it is too early to make a definite assertion. It is suggested, however, that the study of figural after-effects provides a valuable technique for investigating the dynamics of perceptual fields under those conditions in which stroboscopic motion obtains. The technique could easily be extended to study other cases of apparent motion such as the Duncker induced motion type. It would be profitable also, to determine whether continued apparent or real motion of a given type would cause predictable distortions in the subsequent perception of either stationary or moving patterns. In addition, there is some evidence which causes one to suspect that satiation would alter the thresholds for the perception of really and apparently moving patterns. But any attempt theoretically to connect, in the fairly circumscribed area we have selected for this investigation, the dynamics of stroboscopic motion with general perceptual dynamics is obviously premature and awaits more extensive investigation.

The author wishes to thank Professor G. Humphrey for the use of the phi-phenomenon equipment already set up in his experimental room, as well as for his interest and advice. In addition, the author is very grateful to Mr. R. C. Oldfield for his effective aid at many points during the course of the experiment.

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(*Received* 21*st April*, 1950.)

A NEW PHENOMENON OF APPARENT MOVEMENT AND ITS AFTER-EFFECT

BY

J. A. DEUTSCH

(*From the Institute of Experimental Psychology, Oxford*)

It is found that a stationary spiral pattern gives an appearance of movement in a flickering light, and, furthermore, that this apparent rotation gives rise to the same kind of after-effect as a spiral actually rotating. The illusion is obtainable over a wide range of conditions. Detailed results are given in the case of six subjects, but a large number of subjects experienced the illusion. The experimenter has as yet found no one who was not subject to the illusion itself. The after-effects are not universally experienced.

I

Introduction

In 1850, exactly a century ago, Plateau discovered the after-effect of viewing a rotating spiral. He, however, was not the first to note the after-effects of seen movement. In 1834 Addams had published an account of the waterfall illusion. These phenomena attracted considerable attention and gave rise to much controversy, in which Helmholtz and Wundt among others took part. Their discussions are summarized by Boring (1942) and accorded fuller treatment by Wohlgemuth (1911). Controversial interest in this topic has recently revived. With Gibson's (1933, 1937i and 1937ii) discoveries and Köhler's investigations of figural after-effects, attention is again being directed to this field. Fernberger (1948) in a recent note reports an after-effect similar to that of Addams and believes it to be a figural after-effect in three dimensions of the kind described by Köhler and Emery (1947). Also Smith (1948) makes the same identification and criticizes Köhler and Wallach's (1944) satiational theory on this basis.

In the experiment to be reported the dissimilarities between the two classes of phenomena are so marked as to make an identification highly implausible. For it is shown that an after-effect of an impression of movement may occur when the figure of the pattern remains stationary while the ground appears to spin. Köhler's after-effects are on the other hand determined wholly by their figure. The present findings may, it is suggested, also have some bearing on the long-standing problem of the general conditions in which an impression of movement is obtained.

II

Method

(1) *Apparatus:* The apparatus used for the experiment was 12-in. white cardboard disk with a black Archimedean spiral on it, unwinding clockwise. This spiral is a curve traced by a point moving uniformly along a line which at the same time revolves uniformly round a fixed point in itself. The lines of the spiral were about one third of an inch thick and three-quarters of an inch apart from each other. For the intermittent illumination the flash of a high power electronic stroboscopic lamp was used. The rate of flashing was continuously adjustable. For the purposes of accurate description, standard conditions were adopted for six subjects. It is felt, however, that some impressions reported by other subjects who saw the phenomenon might be of interest, and they will accordingly be subjoined.

(2) *Standard Conditions:* These were as follows. A dark-room was used with the spiral 7 ft. away from the seated subject. The stroboscope was on the right side of the subject, approximately 6 ft. away from the spiral.

Condition 1: *Monocular fixation.* The subject was first asked to fixate the centre of the spiral monocularly, i.e. holding a piece of black cardboard before one eye. The period of fixation lasted two minutes.

Condition 2: *Contra-lateral after-effect.* The subject was thereafter requested to shift the cardboard to the other eye while the stroboscope was switched off and a steady light of approximately equal phenomenal intensity was directed at the disk. The subject was asked to report anything he might see.

Condition 3: *Binocular fixation.* He was asked to fixate the centre of the spiral binocularly for the same period (two minutes).

Condition 4: *Tridimensional after-effect.* He was asked to look at a three dimensional object, actually a portrait bust, in steady illumination. The portrait bust was at the same distance as the spiral, but to the right. Thus the subject had to shift his gaze. The rate of flicker which gave optimal movement varied in different subjects from 400 to 800 exposures p.m. (i.e. 6·7 to 13·4 exposures p.s.). Therefore each subject was allowed to fixate the spiral until the optimal rate was discovered. Accordingly the rate of flicker during the fixation period will be reported separately. Movement can in general be seen from 250 exposures p.m. to 1,200 exposures p.m. It has been thought best to give each record separately, on account of interesting variations shown from individual to individual.

In order to avoid the effects of suggestion no leading questions were asked during the course of the experiment. Some of the reports are therefore incomplete.

(3) *Subjects:* These consisted of three students, one male and two female (numbers 1 to 3 in Table I) and three girls, all fourteen years of age (numbers 4 to 6 in Table I). These subjects saw the phenomenon under the conditions described above. Variations in the conditions were made with a larger number of subjects. Any interesting reports from these are included in the general remarks.

III

Results

The main results are given in Table I. The figures give the optimal number of exposures per minute for each subject. Thus, at 600 exposures p.m. the first subject reported white patches rotating with expansion of the spiral and reversing of direction; and slight expansion when the spiral was afterwards viewed with the other eye alone. Binocularly, the impression of rotation was stronger (condition 3). On fixating the bust binocularly he complained of "after image," and then stated that the bust "pulsated (in and out).

Thus, all six subjects obtained an impression of movement, five reported an after-effect when looking at the spiral itself monocularly after fixation with the other eye in the flickering illumination, and four found an after-effect when they looked at the tridimensional test object (a bust of McDougall). The movement in the after-effect is slow.

General Remarks.—The rotation appears sometimes to include the whole disk and sometimes only the white space. The results seem to show that the impression of movement is enhanced either by binocularity or by length of fixation though both

factors are probably operating. Under the conditions described the length of fixation tires the eye and affects sharpness of focussing. As the pattern used was by no means perfect in its fine details a little blurring might be expected to enhance the illusion. A reduction in light intensity has the same effect.

Many people found the flicker tiring and even unpleasant. The frequencies giving optimal movement were all in the range of the main rhythms of brain activity. The discomfort of the subjects (and the experimenter) may be understood better when we consider that in certain individuals these rates of flashing can give rise to various, forms of severe mental and behavioural derangement as is reported by Grey Walter, Dovey and Shipton (1946) and Grey Walter (1950).

TABLE 1

Subject	*Condition 1 (Monocular Fixation)*	*Condition 2 (Contra-lateral after-effect)*	*Condition 3 Binocular Fixation)*	*Condition 4 (Tri-dimensional after-effect)*
1 (male) . .	600 e.p.m. White patches rotating. Expansion. Reversals.	Slight expansion	600 e.p.m. Rotation stronger. Rest as 1.	After-image. Then slow pulsation.
2 (female) . .	400 e.p.m. White patches rotating. Contraction. Reversals.	Expansion and approach.	400 e.p.m. Whole disk rotating. Reversals.	Expansion, approach. Then regression.
3 (female) . .	400 e.p.m. Black rotates, then white.	No after-effect.	400 e.p.m. Polygonal appearance. Complaint of vertigo.	No after-effect.
4 (girl, 14 years)	800 e.p.m. Rotation without reversals.	Expansion.	500 e.p.m. As 1.	No after-effect.
5 (girl, 14 years)	400 e.p.m. Outside rotating, inside still.	Slight expansion	400 e.p.m. Whole disk rotating.	Unsteadiness and movement.
6 (girl, 14 years)	500 e.p.m. Black still. Rotation anti-clockwise.	Expansion. Clockwise movement.	500 e.p.m. Centre going clockwise, outside anti-clockwise.	Bust rocking to and from Cheeks going in and out

Some also reported that the round curves of the spiral became angular. Weitz and Post (1948) and also Weitz and Compton (1950) report a similar transfiguration of a circle into a polygon during prolonged fixation. Those who attempted it could not stop the rotation by taking up an analytic attitude. The movement had for some a quality of unreality. In one case, the only case in which this was done, the movement stopped for the subject when the experimenter gripped the edge of the disk with his hand and started again when he released it. Tilting the disk at an angle did not destroy the illusion. An increase in the rate of flicker produced greater apparent speed of rotation.

IV

Discussion

The after-effects are similar to those of an actually rotating spiral, except that there are reversals in the present case.

For comparison, I quote an account from Bowditch and Hall (1882) of the experiment with an actually rotating spiral.

"In a second series of experiments a large pasteboard disk 22 in. in diameter was inscribed with a black spiral line three-quarters of an in. in diameter, with an equal interval of white and making seven circuits before reaching the circumference. The effects of fixating this disk when in revolution are so striking with some observers as to cause great dizziness and even nausea. . . . If the revolution is in the direction in which the spiral line approaches the centre of the disk the entire surface of the latter seems to

expand during revolution and to contract after it has ceased; and vice versa if the movement of revolution is in the opposite direction. If in the former case the eyes of the observers are turned from the rotating disk towards any familiar object . . . the latter seems to contract or recede in a somewhat striking manner and to expand or approach after the opposite motion of the spiral" (1, p. 299).

We must now consider explanations of this illusion.

1. An eye-movements explanation is contradicted by two facts. First, the movement persists when the flicker rate is too high to permit eye movements during dark periods. Secondly, the illusion does not decrease with proximity as much as might be expected on this explanation. On the contrary, proximity makes very little difference. This is surprising on account of the improved visibility of microstructure, which furnishes a contradictory cue.

2. It may be argued that the different refractory periods of retinal elements cause these to be stimulated in turn by a rapid flicker, thus setting up a rotating process. But it seems implausible that the retinal conditions should be such as in every case to give an impression of smooth continuous movement.

3. The writer would favour a more complicated analysis of the phenomenon. This rests on two facts. First, when the eye is at rest a movement is perceived when there is a quick simultaneous on-and-off stimulation of adjacent groups of retinal cells. But when the eye is moving no movement is perceived, though there is a quick simultaneous on-and-off stimulation of retinal elements. It is therefore inferred that there exists a central mechanism which co-ordinates information from the vestibular apparatus and the extrinsic eye-muscles and from the eye itself. When there is a quick on-and-off stimulation of retinal elements and a compensating stimulation of the vestibular apparatus and the extrinsic eye muscle receptors, the two sets of signals are made to cancel out by the central mechanism. Now real movement and apparent movement have one important item in common. This is a quick simultaneous on-and-off stimulation of adjacent groups of retinal cells not compensated by another definite stimulation of the vestibular apparatus and the extrinsic eye-muscles. Thus no cancellation takes place and we have a direct impression of movement. Under normal conditions it does not operate unless there is an image actually traversing the retina, for obvious reasons. In the present case, however, a quick simultaneous on-and-off stimulation is produced by the flicker; similarly in the phi- and related phenomena.

Second, what is felt to move, and how it is felt to move, is determined by other features of the situation, namely the spatial framework and the properties of the objects seen. Movement will be seen on a body with successive parts similar in shape and size, so that it will not be too difficult to substitute one for another. In the case of the spiral we would thus expect an experience of rotation, this being the most "plausible" movement for such an object.

To test this suggestion, further observations were made. A plain white solid roll or disk of ticker tape was suspended perpendicularly, first of all with only the edge visible, under conditions similar to those in which the spiral was shown. The disk or roll was presented edge on, i.e. with the narrow convex surface towards the observer. The disk appeared to rotate in three dimensions reversing fairly frequently for some observers. Secondly, the disk was perpendicularly suspended with both the edge and the body of the disk visible. The disk was also seen to rotate in this position. If a piece of ticker tape is left hanging loose, and visible to the subject in this latter position, the outermost sheath of the roll appears to be stationary, while

the main body of the roll rotates. It is also possible to produce an illusion of rotation using a pattern consisting of concentric circles. It seems that these findings support the explanation of the apparent movement which has been outlined above.

I wish to express my thanks to Professor G. Humphrey for the facilities afforded me and to Mr. O. L. Zangwill and Mr. R. C. Oldfield for their kindness and encouragement.

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(*Received 13th May*, 1950.)

THE MULTIPLE CRITERION TECHNIQUE OF SUBJECTIVE APPRAISAL

BY

R. G. HOPKINSON

(*From the Building Research Station, D.S.I.R.*)

A technique is described by which the quantitative connection between physical aspects of a stimulus situation and subjective aspects of the perceptual response may be conveniently and reliably determined. The subject is given control of one of the significant physical variables such, for instance, as brightness, and is asked to set this variable to correspond in turn with a limited number of defined criteria relating to a subjective variable such as glare-discomfort. It is found that each criterion acts as a check upon judgments made in terms of the others, so that the scatter of the control settings is less than when a single criterion is used. The functional relationship between the physical and the subjective variable can be estimated, and provided care is taken in the design of the experiment and in the selection of observers, consistent results are obtained.

This technique has been applied during the past ten years to a wide range of visual problems which include those of the visibility of radar echoes, the visibility of street-lighting from the air, discomfort-glare and ease of reading. It is thought that it might find wide application not only in applied psychological work but in the investigation of problems of theoretical import.

I

Introduction

The method to be described arose from a study of discomfort glare in lighted streets initiated in 1937. At that time the available methods for the appraisal of glare in this connection were wholly unsatisfactory. It could be demonstrated by direct experiment, and inferred from the Holladay-Stiles formula (Holladay 1926 and Stiles 1929) that *disability* effect of glare from current designs of street-lighting installations was negligible. But it was clear that the discomfort effects could not be ignored. Holladay (1926) had limited his treatment of discomfort to the "shock" effect of a momentary glimpse of a source, and his characterization of glare-conditions was meaningless in relation to street-lighting. Some attempts to appraise glare-discomfort were being made by lighting installation designers, but these were found to be insufficiently consistent for quantitative treatment. The use of arbitrary scales of discomfort did not in itself improve consistency of judgment.

Experience with a laboratory model experiment in which the various relevant physical variables were under independent control suggested that the vital conditions of obtaining consistent judgment were (1) that the observer should *not* be made to assess the degree of discomfort produced by a given stimulus-situation directly in terms of categories or numerical rating-scale; (2) that, on the contrary, he should be asked to make settings of a physical variable to correspond to a limited number of related criteria of discomfort. It was found that this was a task which the observers could readily and willingly carry out, by contrast with their hesitancy and unwillingness over the inverse method. Moreover the use of a number of related criteria was found to refine judgments in terms of each. Considerable care in the choice of criteria was necessary, however, and it was found that the physical variables were not susceptible of equal ease of adjustment. But the method afforded a quantity

of reliable information as to the factors involved in glare-discomfort, and this information was instrumental in permitting the successful re-design of a number of lighting installations.

Later implications of these principles have borne out its usefulness. Such applications have been made to studies of the visibility of radar echoes (Hopkinson, 1946), the visibility of war-time street-lighting from the air (Waldram and Stevens, 1946), discomfort-glare arising in the lighting of buildings (Petherbridge and Hopkinson, 1950) and the relationship between visual acuity and ease of reading under school conditions (Hopkinson, 1949).

II

Method

The principles of the multiple-criterion technique, and the precautions necessary for its use, were established by trial and error during the course of the glare investigation. The present procedure is summarized below.

Pilot Experiment: It was found that a pilot experiment was always needed to find out which of the variables could best be put under the control of the subject. For example, the relationship between glare-source intensity and surround brightness for constant degree of discomfort glare could theoretically be studied by causing the subject to vary either the source intensity or the surround brightness. In practice, observers felt much more confident when the source intensity was held constant and their task was to adjust the surround brightness for given criteria of discomfort, than when they were given control of the source intensity. Their settings also showed somewhat smaller scatter in the former than in the latter case.

The reasons why observers preferred to control one variable rather than another, and showed more confidence and less scatter in doing so, were not clearly apparent. An investigation of these factors would be of great value.

Control Experiment: It was also found desirable to have a control experiment extending over the whole period of the investigation. In this the subjects made their assessments at regular intervals (twice daily, for example) on a set of standard conditions. This control experiment had a number of functions. In the first place, it enabled the subject to gain confidence in his ability to make the assessments. Secondly, it provided the experimenter with the opportunity of detecting "off-days" when a subject's settings would wander significantly away from their mean position. Thirdly, it permitted an analysis to be made of the scatter of any subject's observations under standard conditions, so that confidence limits could be derived from his observations under similar conditions. Figure 1 shows a typical plot of one observer's readings under standard conditions in assessment of discomfort glare (Petherbridge and Hopkinson, 1950). The standard deviation of this set of observations is 0·16 on the log. foot-lambert scale, and the coefficient of variation is 0·45.

In general, it will be seen, this observer keeps to his mean assessment fairly consistently, but occasionally wanders well away from the mean (for example on the 18th, 36th and 73rd days). On such days it was considered inadvisable for him to make assessments in the experimental series, especially if his observations were to be pooled with those of other observers.

Selection of Observers: At an early stage of the glare experiment it was decided to conduct the more difficult parts of the investigation with the aid of those observers only whose settings were reasonably consistent. Since, in this experiment, a brightness ratio of 4:1 resulted on the average in a change of one step in the criterion of discomfort, it was decided that an observer should be rejected if he made a setting

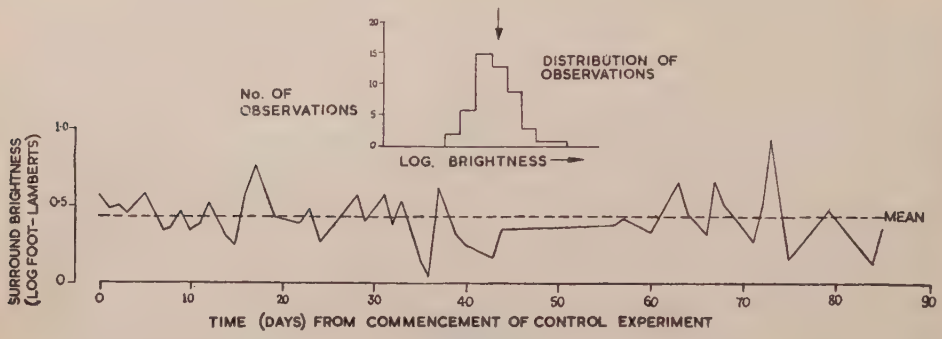

FIGURE 1

TYPICAL SET OF OBSERVATIONS OF A "CONTROL" EXPERIMENT. ASSESSMENT OF SURROUND BRIGHTNESS WITH WHICH GIVEN GLARE SOURCES GIVE "JUST UNCOMFORTABLE" GLARE

outside the limits twice the mean, and half the mean, more than once in twenty times. (This corresponds to a coefficient of variation of 0·42). Hence only once in twenty times would the setting for one criterion fall into the range of settings for the adjacent criterion. This procedure was followed in the subsequent studies, a few consistent observers being preferred to a larger number with lower consistency. As far as possible, staff who were associated with the results of the investigation were not themselves employed as observers. Fig. 1 is a typical plot for a consistent observer performing the control experiment. Fig. 2 shows a plot, under the same conditions,

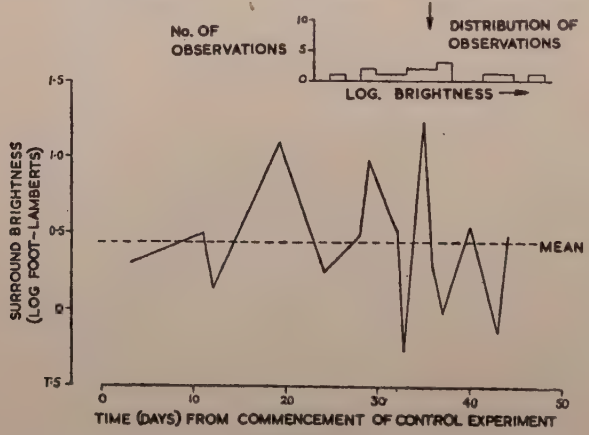

FIGURE 2

ASSESSMENT MADE BY INEXPERIENCED OBSERVER
(Compare with Figure 1)

of a less consistent one. Fig. 3 shows a set of results by a consistent observer, Fig. 4 by an inexperienced and less consistent one.

It was found that observers varied considerably in the amount of confidence they felt and showed in their settings, and that in general those with the less confidence showed the greater scatter. In some cases the individuals who showed uncertainty could be induced to "guess," and such guesses often resulted in a surprising consistency of setting. If such an individual was now shown that his performance was not as poor as he expected, his confidence was assisted, and in this sense he could be "trained" to become a "useful" observer.

Choice of Criteria: As the technique was extended to a wider range of problems, it was found that the value of an experiment could be increased by careful attention to the choice of criteria. In particular, the form of words used in the description of each criterion had to be one which was concretely and directly applicable to the observer's own experience. For example, the set of criteria "just visible," "moderately visible," "reasonably visible," "easily visible," would be less satisfactory than "just visible," "just recognizable," "pattern just identifiable," "pattern definitely identifiable," An experienced observer could work with the first set, allocating to

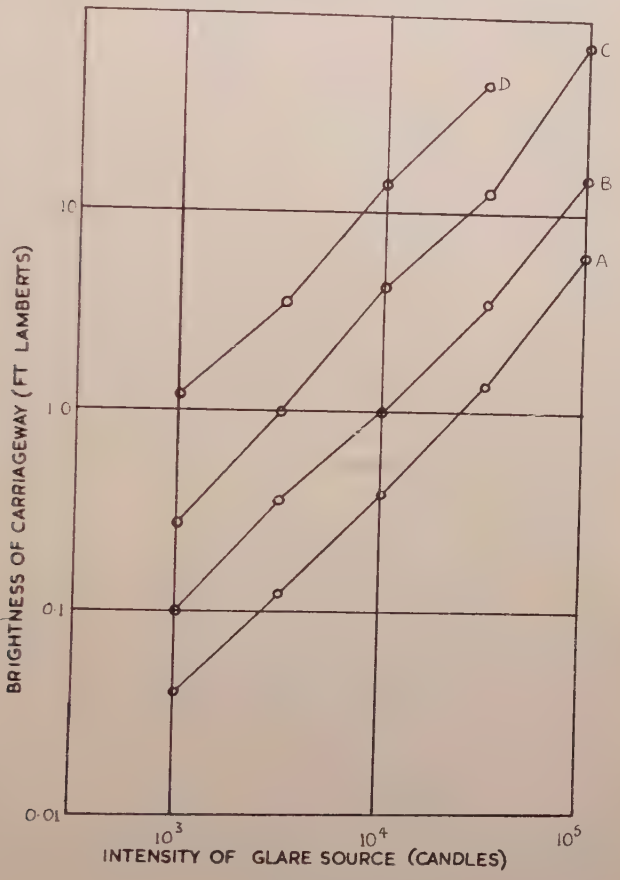

FIGURE 3

TYPICAL SET OF OBSERVATIONS SHOWING ASSESSMENT OF CARRIAGEWAY AND SURROUNDING BRIGHTNESS IN RELATION TO INTENSITY OF STREET-LIGHTING LANTERNS, FOR FOUR DEFINED CRITERIA OF GLARE (SHOWN BY A, B, C, D on DIAGRAM)

each degree of "visibility" a mental assessment of the degree of identification of the pattern. But a less experienced one would not be able to make such an interpretation in a consistent way, and would be uncertain as to the exact meaning of "visible."

An appropriate form of words did, in fact, generally emerge from the pilot experiment If the criteria were not well defined, observers would express doubts as to what was required of them. More formal investigation of the effect of criteria used on consistency of judgment would be of considerable interest and value.

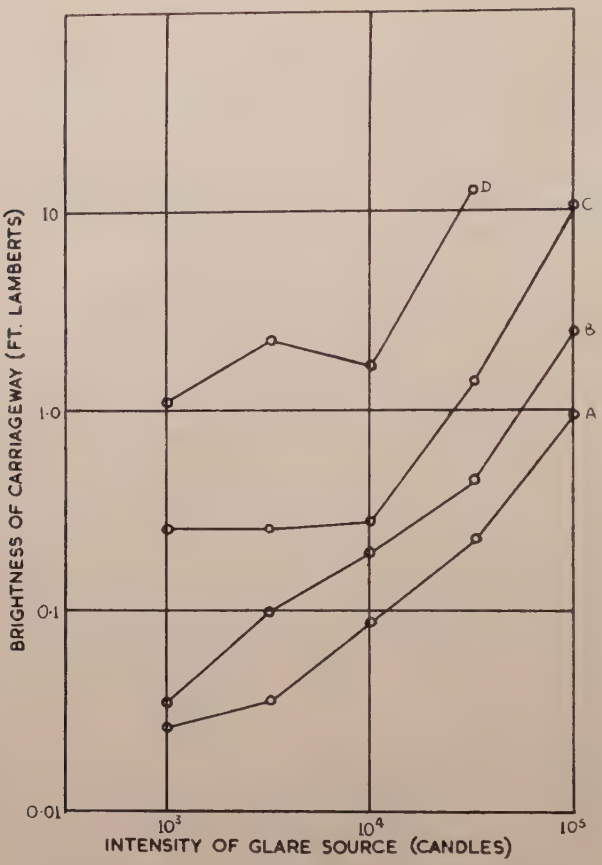

FIGURE 4

ASSESSMENTS BY INEXPERIENCED OBSERVER

(Compare with Figure 3)

III

EXPERIMENTAL PROCEDURE

The type of experimental procedure employed is perhaps best indicated by summarizing briefly the sequence of events in one experiment on radar trace visibility.

1. The observer enters the room, in which the general brightness level is about 0·1 foot lamberts. The lighting is indirect and free from glare and distracting features.

2. The recorder informs the observer that he is to make observations on the visibility of a radar trace on the face of the cathode-ray tube in front of which he is seated. The trace has a certain definite characteristic with which he is acquainted from previous observations under similar conditions.

3. A preliminary period of about five minutes is allowed during which the observer sits quietly and adapts to his surroundings. At the end of this time, the recorder sets the background brightness on the face of the cathode-ray tube to the first level to be studied.

4. The recorder then asks the observer to take control of the signal brightness. He is told that, as he raises the signal brightness, he will first become aware only of the existence and position of an area of higher brightness than the background, but will be unable to recognize its pattern.

At this point he is to call out "just visible." He is then to raise the brightness of the signal slowly until he reaches the point at which he judges that he can just recognize the characteristic pattern of two closely juxtaposed double traces. At this point he is to call "just recognizable." On further raising the brightness he is to call out "measurable" when he is quite sure that he can assess the relative lengths of the two double traces. Finally he is to raise the brightness to the lowest level at which in his opinion the signal is easily visible for continued working, and call "easily visible." Any further explanation of the criteria is given if required. When he fully understands the instructions, the observations commence.

5. The observer is free to take as long as he wishes. No assistance is given him unless he experiences difficulty or lack of confidence. The recorder logs the observations at each stage without comment. The observer has no indication of the values of his settings, and the controls are so arranged (e.g. by series resistance in a lamp brightness circuit) that the position of the control knob, or the angle through which it is turned, give no clue to the values reached.

6. The series of four criteria being completed at the first level of surround brightness, the observer has a short rest of about a minute while the next setting of surround brightness is set up by the recorder.

7. The process is repeated at the other surround brightness values.

A session will last from about ten minutes with an easy assessment, to about thirty minutes with a difficult one. The incidence of consciously felt fatigue is avoided.

IV

Discussion

The validity of such a technique as that described must, in the first instance, be judged by the success of operations based on the conclusions to which its use may lead. On this score there is some evidence of its usefulness. Two independent teams of experimenters, working in one case on problems associated with the visibility of radar displays (Hopkinson,1946), and in the other with a wide range of war-time visibility problems (Waldram and Stevens, 1946), found that reliable conclusions were reached from experiments which used the technique. It was from such work that decisions regarding agreed levels of war-time street-lighting were influenced. New designs of street lantern were evolved, too, on the basis of appraisals made by the technique, and these were found in practice to be more satisfactory than the old.

Another kind of check introduces a point of some theoretical interest. In certain cases it was possible to have small groups of observers, who had participated in the laboratory studies, make appraisals in actual streets, while at the same time measurements of the prevalent conditions could be made comparable with the laboratory set-up. In this case observers were asked to express the amount of discomfort experienced in terms of the criteria used in the laboratory experiment. It was found that observers who had been made familiar with the assessment of discomfort in terms of four fixed criteria were no longer reluctant to make *direct* assessment in terms of these criteria. The agreement between direct appraisal in actual streets, and indirect in a laboratory set-up similar as regards the physical variables involved was found to be good (Hopkinson, 1940, Table X).

Thirdly, it has been possible in some cases to check the appraisals against objective criteria of functional efficiency. In the work on the influence of brightness and size of radar echoes on ease of recognition, for instance, direct measurement was also made of percentage of accurate localizations. It was found that the variation between subjects in the mean percentage of accurate locations, corresponding to the settings of the variables for a given criterion of ease of recognition, was quite small, and amounted to about 5 per cent. This suggested that each criterion of ease of recognition did in fact correspond to a real standard of performance.

More recently, tests on the effect of illumination and contrast on ease of reading have been made in which the multiple criterion technique has been combined with measurements of speed of reading. The observers who were employed in the tests were selected for certain characteristics of their vision, and not for their intelligence or ability to operate the technique. The variation of their assessments might therefore be expected to be greater than that of the picked observers of the radar experiment. Table I summarizes some of the observations that were obtained.

It can be seen that "barely readable" and "reasonably readable" correspond to mean speeds of 0·4 and 0·7 of the maximum speed respectively. The coefficient of

TABLE I

VARIABILITY* OF MEASURED VALUES OF SPEED OF READING (EXPRESSED AS A RATIO OF MAXIMUM SPEED FOR EACH OBSERVER) FOR TWO FIXED CRITERIA OF EASE OF READING. (EACH OF 20 OBSERVERS GAVE ONE VALUE OF READING SPEED FOR EACH CRITERION OF EASE OF READING, UNDER EACH OF 12 CONDITIONS OF ILLUMINATION OR CONTRAST.)

	Ease of Reading			
	Criterion B "Barely Readable"		*Criterion C "Reasonably Readable"*	
	Speed of Reading Mean of 20 Values	*Variability about Mean*	*Speed of Reading Mean of 20 Values*	*Variability about Mean*
Condition 1	0·35	39·5%	0·69	17·9%
2	0·36	26·2%	0·67	21·9%
3	0·34	22·1%	0·71	18·4%
4	0·31	36·1%	0·62	22·3%
5	0·40	28·8%	0·73	17·4%
6	0·43	25·0%	0·76	18·0%
7	0·41	34·1%	0·67	19·6%
8	0·40	37·2%	0·70	20·8%
9	0·43	38·0%	0·75	14·7%
10	0·39	28·2%	0·68	12·5%
11	0·41	28·3%	0·71	14·2%
12	0·41	23·1%	0·77	15·6%
Mean for all 12 conditions	0·39	—	0·70	—
Variability of the means for each condition about the mean for all conditions		10%	—	6·8%

$$\text{*Variability} = \frac{\text{standard deviation}}{\text{mean}} \ 100\%.$$

variation of the means for each of the 12 conditions, with respect to the mean of the whole of the 12 conditions is about 0·08. That of the individual values for each

observer about the mean for any one condition is about 0·25. These figures suggest that even observers inexperienced in the multiple criterion technique can, by a suitably designed experiment, give assessments of reasonable accuracy, which link with comparable performance figures.

It must be pointed out, however, that the particular coefficient of variation found in each task is dependent upon a number of factors. Among these we may distinguish the inherent difficulty of the judgment required, from the suitability of any given experimental design (in terms of variable chosen for control, formulation of criteria, etc.) to facilitate the making of the judgment. Thus, if we find a coefficient of variation of 0·05 on a search task, and one of 0·20 on a reading test, this difference may in part be due to an inescapable difference of inherent difficulty in the two tasks, and only in part to a corrigible difference in experimental design. Further work is needed to establish the relative magnitude of these factors so far as may be possible. It might then become possible to predict, to some extent, the consistency to be expected of any particular experiment. A high coefficient of variation should not of itself cause the rejection of an experiment, because it may still yield information of value, which cannot be obtained in any other way.

The scatter of the separate means for each observer about the mean of all these means is not easy of interpretation. Part of the variance may be attributable to random experimental errors common to all observers, while another part may be due to real differences in the psychological characteristics of different observers. Conditions which may be acceptable to one observer may not be tolerable to another more sensitive observer. It is not possible to determine whether or not the subjects have all interpreted the criteria in the same way. Direct experiment can penetrate only to a limited extent into the nature and magnitude of the sensory reactions of an observer.

V

Acknowledgments

The work described in this paper was first undertaken when the author was with the Research Laboratories of The General Electric Company Ltd., Wembley. The recent work is part of the programme of the Building Research Board, and this paper is published by permission of the Director of Building Research, Department of Scientific and Industrial Research. The author gratefully acknowledges the help of his colleagues in the development of these investigations.

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(*Received 5th August*, 1949.)

THE INFLUENCE OF AN INTERPOLATED EXPERIENCE UPON RECOGNITION

BY

D. RUSSELL DAVIS and DURGANAND SINHA

(From the Psychological Laboratory, University of Cambridge)

It was demonstrated in a previous experiment that an experience interpolated between an original experience and its recall may bring about changes in the points of emphasis in the recall of the original experience. Moreover, details of the interpolated experience may be recalled as if they had formed part of the original experience. These results were taken to mean that two experiences of a related kind may become merged in memory into something akin to Bartlett's notion of an organized mass of past experiences. In the experiment here reported, the original experience was the hearing of a story, and the interpolated experience the seeing of a picture which illustrated part of the story. When in a recognition test subjects were asked to select from three alternatives(including the original) the one version which was "most like the original story," a proportion of them preferred *to the original story* a version which differed from the original by including a number of details from the picture. Asked about details, all the subjects tended to place details from the picture in the story, even if they had not been mentioned there. The results of the two experiments are thought to show that irreversible changes are brought about in the memory of an experience by subsequent experiences of a related kind.

I

Introduction

We have recently reported an experiment in which the recall of one experience was shown to be modified by a second experience of a related kind (Davis and Sinha, 1950). This experiment was one of a series having the form:

Group	First Session: *Original Experience*	Second Session: *Interpolated Experience*	Third Session: *Description, Recall or Recognition Test*
1	Experience A	Experience B	Experience A
2	,, A	—	,, A
3	,, A	Experience B	,, B
4	—	,, B	,, B

In the experiment already reported, the material forming the original experience was a story, and that forming the interpolated experience a picture which illustrated more or less accurately a part of the story. These two experiences were shown to interact. Thus the perception and recall of the picture (as judged by the comparison of the results obtained in the third session from Groups 3 and 4) were governed by the attitudes induced by the story. These attitudes directed attention to, and prescribed the interpretation of, those details in the picture which played an important part in the theme of the story. Thus perceived, the picture influenced the recall of the story (as judged by the comparison of Groups 1 and 2). Seeing the picture brought about changes in the points of emphasis, favouring the recall of those aspects of the story which the picture illustrated. Details of the story not reinforced by the picture tended to be left out. Many picture details intruded into the reproductions of the story; that is, many details which were part of the interpolated, but not of the original experience, were recalled as if they had been part of the original.

These findings were taken as confirmation of Bartlett's notion that memories are retained as *schemata* or active organizations of related experiences, which are subject to modification or reconstruction as a result of new experiences of a relevant kind (Bartlett, 1932). Bartlett implied that the changes which memories undergo are irreversible, and that memories lose their separate identity and become incapable of recall in their original form. On the other hand, so far as they are pertinent, the findings argue against Freud's contention that in some way or other memories survive unchanged in the mind, as a rule rather than as an exception; thus Freud wrote in 1930, ". . . we have been inclined to the opposite view that nothing once formed in the mind could ever perish, that everything survives in some way or other, and is capable under certain conditions of being brought to light again, as, for instance, when regression extends back far enough." . . . "The fact is that a survival of all the early stages alongside the final form is only possible in the mind." (Vide Freud, 1930, pp. 15–20.)

In contrast to Freud, experimental psychologists have in general asserted that in whatever form they are retained, memories become modified with lapse of time, as a result either of the intrinsic stresses arising from their configurational characteristics or of the extrinsic stresses resulting from antecedent or subsequent experiences. Indeed, many experimental studies have been carried out with the purpose of defining the changes to which memory traces or *engrams* are subject. Two types of data can readily be obtained: those from recall or reproduction tests, and those from recognition tests, these data being assumed to reveal changes in the engrams themselves. This assumption is valid only within limits, however, for it is often belied by the ancillary evidence to some extent; for instance, recognition tests sometimes yield results different from those yielded by recall tests; again, introspections often suggest that the material recalled does not fully represent the engram. Moreover, those who wish to insist upon the validity of Freud's theory of memory may treat the changes observed in recall or recognition tests, not as evidence of changes in the engrams, but merely as changes in the operation of the mechanism of repression.

Nevertheless, there are good reasons for the careful study of the results of recall and recognition tests, although they may have to be interpreted with caution. Accordingly we have examined the results of recognition tests in experiments of the above design, to discover whether they show the same degree of confusion between original and interpolated experiences as was shown by the recall rests described in our previous paper.

II

Method and Procedure

Of the several methods available, that of successive or repeated reproduction has been the most widely used in research on these problems. It is especially applicable when the autonomous changes due to intrinsic stresses are studied (e.g. Zangwill, 1937). Yet it has certain disadvantages in the study of extrinsic stresses, for in it there is no control or manipulation of the other related experiences from which these stresses are held to arise. Bartlett applied it in many of his most interesting experiments and was able to demonstrate in a convincing manner that recall was modified by rationalization and conventionalization, which represented the effects of antecedent experiences, but he was not able to show in any precise way how these effects came about. Zangwill (1939), too, used it in an experiment similar in some respects to the one to be reported below. Some of the disadvantages of this method have been avoided by the method of interpolated experience, which we have used.

In the experiment to be reported, the material forming the original and the interpolated experience was essentially the same as in the previous experiment. The original experience was the hearing of a shortened version of the story (about 500 words in this experiment) which was written for the purpose, and the interpolated experience was formed by Pieter Breughel's picture, "The Village Wedding." The story gave a new significance to the scene of festivity portrayed in this picture, its first part describing a feud between two peasant families which culminated in their reconciliation by the betrothal of the son of one family to the daughter of the other, and its main part a wedding feast disturbed by several untoward incidents.

This story was read out to forty university students, divided between Groups 1 and 2. The findings in Groups 3 and 4 are not pertinent to the present enquiry. Twelve days later half of them (Group 1) were shown individually a set of seven postcards, from which they were asked to pick out the one relevant to the story. All the postcards were coloured reproductions of pictures. Everyone selected "The Village Wedding," for reasons which are discussed in the previous paper, and was then given a short time to examine it. The postcards were not shown to the subjects of Group 2.

After an interval of from five to six weeks, all the subjects of Groups 1 and 2 were given (i) a recognition test of the story, and (ii) a questionnaire test. Comparison of the results in the two groups reveals the effects in Group 1 of the interpolated experience.

(i) In the recognition test, three versions of the story were read to each subject who was then asked to rank them in order of their similarity to the original story. The order in which the versions were read was controlled. Subjects were also asked to rate the certainty of their ranking, and to mention the factors which determined it. Finally, typed copies of the versions were given to them to read, and notes were made of their further comments.

Version A was the original story, unaltered.

Version B was similar to the original story in style, form and length. Its first part was unaltered, but the main part describing the wedding feast included several details present in the picture, but not in the original story; some details from the original story were omitted and this version provided a better account of the picture than did the original story.

Version C differed considerably from the original in detail, although similar in length and in theme.

(ii) Both questions and answers in the questionnaire test were written. The test comprised 35 questions, each asking whether a detail had been mentioned in the original story. An answer, "Yes," "No" or "I don't know," was invited to each. Three types of detail were asked about:

A details, present in the original story;

B details, not present in the original story, but present in the picture;

C details, present neither in the original story, nor in the picture.

The questionnaire test was applied after the recognition test. Some B details were contained in Version B, others were not. Some C details were contained in Version C.

III

Results

(i) *Recognition test.*

Version C was rejected by all subjects. Eighteen of the subjects of Group 2 ranked Version A as most like the original; 11 of these stated it to be the original.

Two subjects of this group were doubtful whether to prefer Version A or B; after reading the versions for himself, one remained doubtful, and the other identified Version A as the original.

Five subjects of Group 1 ranked Version B as the most like the original; 14 preferred Version A, and of these, 10 identified it correctly as the original; one subject was very doubtful, regarding Version B as more like the original than Version A, except for one or two details; after reading the versions for himself, however, he preferred Version A.

Those subjects of Group 1 who ranked Version B as the most like, did so with less confidence than that with which the majority of the rankings were made. On the other hand, some of the subjects of Group 1 who preferred Version A were dissatisfied with this ranking; for instance, three such subjects remarked that details present in the original had been left out of Version A: "Some sort of reference to servants carrying food is absent in A." "In A villagers sitting on frail wooden benches have been left out." "There was something about music, though no musicians were explicitly mentioned. That is also left out in A." All the details mentioned in these remarks are properly picture details which were not present in the original story. It was noted in this connection that subjects of both groups tended to regard Version A as less detailed and more condensed than had been the original, but it was only these three subjects of Group 1 who mentioned details which (they thought) had been left out.

That five subjects of Group 1 should have preferred Version B to A shows that recognition of an original experience may be affected by an interpolated experience. It is the more striking, since the details which were introduced into Version B from the picture were necessarily expressed in unfamiliar phrases. Indeed Version A was often preferred by subjects of Group 1 because the phrasing was familiar, e.g. subjects said of Version A: "Literary style the same—lots of sentences were familiar." "The tone of A struck me as identical with that of the original and every detail of description and action seemed familiar." "The gay chatter of the bride . . . their faces showed only a childish yearning for food as saliva dribbled from their mouths'—in fact, I was waiting for these phrases." Similarly, the unfamiliarity of phrasing in Version B was a main reason for its rejection by at least four members of Group 1. Again, several others of this group rejected Version B because it omitted details remembered in the original.

Selection or rejection of a version thus depended in many cases upon the presence or absence of a small number of details. For this reason the recognition test was less satisfactory than might have been supposed.

(ii) *Questionnaire test.*

The two groups did not differ in their answers to the questions concerning A details, about 70 per cent. of the questions receiving affirmative (correct) answers, about 20 per cent. negative (wrong) answers and about 10 per cent. "I don't know" answers. Similar results were obtained for C details. For B details, the following results were obtained:

	Affirmative	*Negative*	*I don't know*
Group 1	90	100	30
Group 2	47	136	37

(There were 20 subjects in each group, and 11 questions concerned B details.)

In the case of B details, affirmative answers were incorrect. That Group 1

should have given a significantly larger number (P being less than 0·01) of such answers than Group 2 indicates that details of an interpolated experience *intrude* into the memory of an original experience.

IV

Discussion

Zangwill (1939) has reported a similar experiment, without the deliberate use of an interpolated experience. His subjects reproduced a prose passage three months after it had been read—a much longer time interval than in our experiment. This reproduction was used to construct a version including the salient inaccuracies in recall. Four or possibly five of his seven subjects expressed some difficulties in deciding in a recognition test, whether this version or the original story was the original and suggested that the original story should contain some of the inaccuracies present in the reconstructed version. Our results were similar, as was the experimental method, except in so far as we brought about the modification of the memories of the original experience experimentally, and Zangwill allowed the modification to come about with the passage of time.

The results of the present and the previous experiment allow the interpretation that an interpolated experience brings about changes in the recall or recognition of an original experience, accurate recall or recognition becoming impossible. It is not to be supposed, however, that there is complete compounding of the two experiences. On the contrary, they seem to retain their separate identities to some degree; some details, for instance, remain capable of correct recall or recognition as derived from the original experience, others as derived from the interpolated experience; only some details are confused, intruding from one experience into the other.

If those details which intruded from the interpolated into the original experience governed recognition of the original experience entirely, Version B would have been preferred invariably, or almost invariably, in our experiment. But this was not the case, as the reports of our subjects made clear; preference in the recognition test was determined by other factors in the case of many of the subjects, and depended upon factors which had not been influenced by the interpolated experience or had not been adequately controlled in the preparation of Version B. Again, modifications might well have been brought about in the memory of the original experience, which did not correspond to those introduced by the experimenter into Version B. The hypothesis that an interpolated experience brings about changes in the recognition of an original experience is not disproved by the preference for Version A shown by the majority of the subjects in Group 1., on the other hand, a hypothesis of independence of the two experiences would seem to be disproved by the preference by a minority of Version B.

The questionnaire test provided a more straightforward proof of a more restricted hypothesis that the memory of an original experience becomes modified by the intrusion of details from an interpolated experience. The results show that at least some of the intrusions which had been predicted came about.

Taken together, the results of the recall experiment reported previously and of this experiment indicated that, under the conditions obtaining, memories of related experiences do become merged to some degree, in a manner corresponding to Bartlett's notion of an "organized mass" of past experiences. There is no evidence immediately available to show that the merging is reversible. Probably it is not, any apparent reversal being due, not to a separation of the two experiences, but to a reconstruction by inference.

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(*Received 20th February*, 1950.)

MISCELLANEA

NOTE ON THE CORRELATION OF ERRORS AND PERFORMANCE IN A TEST

BY

J. W. WHITFIELD

(*From the Medical Research Council Applied Psychology Research Unit, Cambridge*)

In the common choice-response type of intelligence test, if an individual's intellectual ability fails him for a specific question he has the choice of omitting the question or of hazarding a guess. A similar choice is open in a number of experimental learning problems. By chance he will guess correctly in some cases, more often he will be in error. It is highly likely that individuals differ in their willingness to guess; this difference may or may not be related to intelligence or test sophistication, or such a relationship, if found, might be susceptible to change in test conditions.

A pure measure of "guessing" is out of the question, but apart from "catch" questions, the number of wrong answers given can be considered as an impure or indirect measure of guessing. There is no way of deciding which correct answers were in fact guessed. Thus the guessing evidence consists of the number of wrong answers and the number of questions unanswered. These two are, however, constrained by the total number of questions in the test and the total number of correct responses. It is obvious that an individual who correctly answers nearly all the test questions can demonstrate very little guessing, as it has been defined, whereas one with few correct answers can show many guessed answers.

Let us consider a test of "n" questions and a population N attempting the test. Let the mean number of questions answered correctly be C, and the corresponding incorrect (i.e. errors and unanswered) be I ($C + I = n$). Let the mean number of errors be E, and let the incorrect (or correct) answers be distributed about their mean with standard deviation σ. If there is no variation in guessing propensity amongst subjects, then for individuals who show x incorrect answers the errors will be distributed according to the binomial expansion $(p + q)^x$ where $p = \frac{E.}{I}$

Thus for the whole range of possible incorrect scores we have the following probability table:—

Number of Incorrect Answers i	P	*Mean Number of Errors* e	*Variance of Error Distribution*	*Product Term (Incorrect x Errors* $= ie$
0	P_0	0	0	0
1	P_1	p	$p\,(1-p)$	p
2	P_2	$2p$	$2p\,(1-p)$	$4p$
3	P_3	$3p$	$3p\,(1-p)$	$9p$
.	.	.	.	.
.	.	.	.	.
.	.	.	.	.
n	Pn	np	$np\,(1-p)$	n^2p

$\Sigma P = 1,\ \Sigma Pi = I,\ \Sigma Pe = E = pI$

$\Sigma Pi.e = p\sigma^2 + pI^2$ {i.e. $p\ \Sigma P(i)^2$}

$$\begin{aligned}\Sigma P(e)^2 = \quad & p^2\{P_1 + 4P_2 + 9P_3 + \ldots\ n^2Pn\} \\ & + p\ \{P_1 + 2P_2 + 3P_3 + \ldots\ nPn\} \\ & - p^2\{P_1 + 2P_2 + 3P_3 + \ldots\ nPn\} \\ & = p^2\{\sigma^2 + I^2\} + pI - p^2I\end{aligned}$$

$\therefore \mathrm{Var}\, e = p^2\sigma^2 + pI\,(1-p).$

This expected variance itself provides data upon which to consider test behaviour. If individual differences in proneness to guess exist, and are uncorrelated with test score, the observed variance of the error distribution will exceed that calculated by the above formula. If, in addition, there is a correlation in the direction of the poorer the test score, the greater the likelihood of guessing, then the observed variance is considerably increased. If, however, the correlation is in the reverse direction, i.e. the better the test score the greater the number of guesses, then the variance will tend to be reduced, particularly if the high scores on the test are fairly near to the maximum possible performance.

The expected regression of e upon i has slope p, and the observed and expected regressions can be drawn to aid inspection. Alternatively the observed and expected product moment correlation between i and e can be calculated.

$$\Sigma \text{P}.i.e. \text{ is } p\sigma^2 + p\text{I}^2.$$

The expected value of r is therefore—

$$= \frac{p\sigma^2 + p\text{I}^2 - \text{I}p\text{I}}{\sigma\sqrt{p^2\sigma^2 + p\text{I}\,(1-p)}} = \frac{p\sigma}{\sqrt{p^2\sigma^2 + p\text{I}\,(1-p)}}$$

It will be seen that the expected correlation between the number of incorrect and the number of errors will always be positive, tending towards unity as p tends towards 0 or 1, and, for given values of I and σ it will be at a minimum when $p = \frac{1}{2}$.

The relation between the observed $i\,e$ correlation, and that expected on this null hypothesis, taken in conjunction with the observations on the variance provides information on the behaviour of individuals in the intelligence test situation.

(*Manuscript received April,* 1950.)

A SIMPLE CLASS-ROOM CHRONOSCOPE

BY

A. CARPENTER

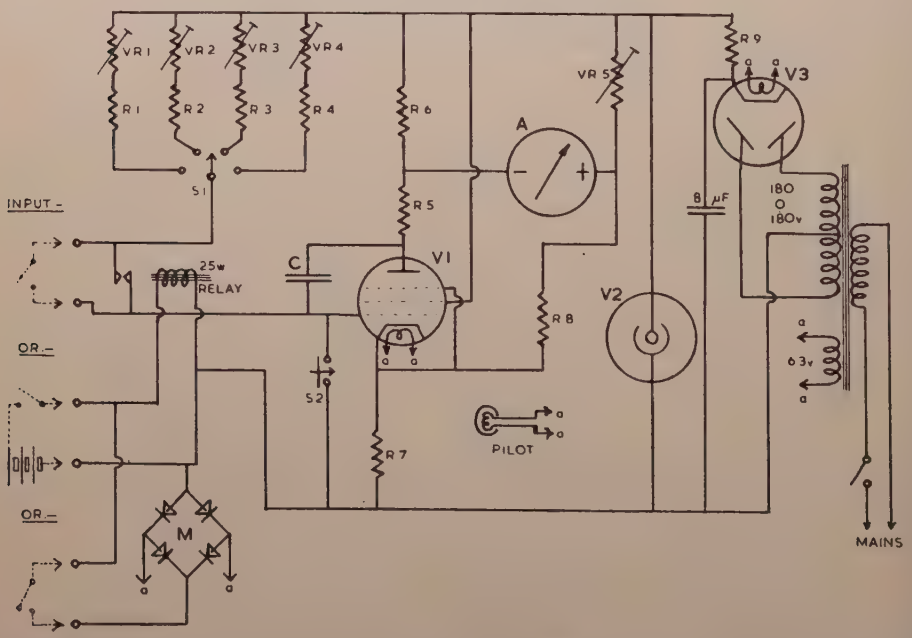
(*From the Medical Research Council Applied Psychology Research Unit, Cambridge*)

THIS note describes an electric chronoscope which is simple and cheap to make, and which while not pretending to a high order of accuracy, is adequate for the majority of classrooms, and many research purposes.

The chronoscope described here was made in response to a request for an apparatus simple enough to operate and consistent enough in its readings for routine use by a class of students; yet sufficiently simple in design and construction, for a number of them to be made quickly. It was desired to measure the time for which an electrical circuit was closed, over a range of approximately 0·1 to 5 seconds, and it was decided that an accuracy of the order of 2 per cent. of the full-scale reading would be adequate. The choice was between a mechanical system operated by a constant speed motor, relying upon the accuracy of the frequency of the electric mains supply, and using some form of magnetic clutch to engage the scale pointers with the drive, as in the Hipp, and an

FIGURE 1

CIRCUIT DIAGRAM OF CHRONOSCOPE



electric system relying upon the charging of a condenser from a constant voltage source. The latter was decided upon for two reasons: (*a*) for consistent operation, a mechanical system would require to be constructed with considerable care and skill, and would therefore be costly, and (*b*) recent developments in valve-operated electrical circuits have made the latter method practicable and simple. Using standard small radio components it is possible to complete this instrument within two days, and for a cost of the order of £5, plus the cost of a suitable meter.

The circuit is given in Figure 1. It will be seen that the condenser, C, is charged from a voltage derived from the mains in the usual way, and stabilised with a gas discharge valve V_2. Four ranges are provided, corresponding to full-scale readings of 0·5, 1, 2 and 5

seconds, and are selected by the four-position switch S_1, which selects the resistor through which the condenser is charged. These resistors each consist of standard 1 watt carbon components R_{4-1}, in series with pre-set carbon potentiometers, VR_{4-1}. The accurate setting of these pre-set controls determines the absolute accuracy of the readings obtained, but for those purposes for which comparative readings are adequate, it is unnecessary to set these controls accurately. The condenser, C, is placed between the anode and control grid of the valve V_1, thus making use of the Miller effect to obtain linearity of charging rate. With the valve and component values given the grid voltage actually changes by about 2 volts, so that at full-scale reading the rate of charge is just more than 1 per cent. less than the initial rate.

The insulation of the grid circuit of V_1 is of the highest importance in avoiding drift of the meter reading when the operating circuit is open. For this reason the input terminals are mounted on an insulating panel of Paxolin, and also, to enable the instrument to be connected to external apparatus whose insulation may be imperfect, a high-speed relay, operated either from the valve low tension supply, or from an external source of some 6 volts, is included. The 4 microfarad condenser, C, must be chosen to have as low a leakage as possible. In the instrument made the drift was unreadable over a period of five minutes.

The valve used as V_1 is not critical. A high mutual conductance is an advantage, and a top grid connection is desirable to minimise leakage.

The meter, which reads the anode current of V_1, determines the accuracy of the complete instrument. In the first model made this was a panel mounting instrument with a $2\frac{1}{2}$ in. scale, the whole being made as one unit, but in later copies larger meters have been used, connected externally. There is no reason why the meter should not be several yards distant from the rest of the instrument. Zero is adjusted by VR_5, the minimum valve current to be backed off by this means is about 0·5 mA.

Switch S_2 is a push-button switch which on closure restores the meter reading to zero by allowing C to charge almost to the full H.T. potential, through R_5.

Possible sources of inaccuracy in this instrument are: (*a*) faulty setting of VR_{1-4} will produce absolute error of calibration; (*b*) faulty zero setting of VR_5 will produce a constant error; (*c*) if used on low ranges the operating time of the relay may introduce a constant error; (*d*) non-linearity due to exponential charging of C. As mentioned before, this is about $1\frac{1}{2}$ per cent. in this instrument; (*e*) leakage causes slow drift of the meter reading. The meter should be returned to zero just before taking each reading, and read without undue loss of time. In this instrument, drift was negligible. Finally, attempts to assess unreliability due to random causes have shown that readings can be relied upon to be repeatable to within the error involved in reading the meter scale—less than 1 per cent. of the full scale reading.

This circuit involves no new principles or methods. Fuller information will be found in the references given.

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Component Values

V_1	—	SP61	R_1	—	150 K.
V_2	—	VR150	R_2	—	250 K.
V_3	—	6 X 5	R_3	—	500 K.
A	—	0–5 mA, 25 *w*.	R_4	—	1 M.
C	—	4 microfarad high insulation	R_5	—	15 K.
M	—	0·25 A. metal rectifier	R_6	—	220 *w*.
VR_1	—	50 K.	R_7	—	270 *w*.
VR_2	—	100 K.	R_8	—	15 K.
VR_3	—	500 K.	R_9	—	2·5 K.
VR_4	—	1 M.	VR_5	—	30 *w*.

BOOK REVIEW

The Organization of Behaviour: a Neuropsychological Theory. By D. O. Hebb. New York: J. Wiley & Sons; London: Chapman & Hall, Ltd. Pp. xix + 335. 32s. net.

In this lively and intelligent book, Dr. Hebb has tried to develop some broad principles of nervous organization which might have explanatory value in psychology. His aim throughout is to find conceptions ". . . which will be valid physiologically and at the same time 'molar' enough to be useful in the analysis of behaviour" (p. 11). If the task is difficult, Hebb has none the less made an original and exciting beginning. In the stagnant atmosphere of contemporary psychological discussion this little book comes like a breath of fresh air.

The earlier chapters are concerned in part to demolish current formulations and in part to suggest more acceptable alternatives from the standpoint of neurophysiology. First, the notion that behaviour is (or can ever be) completely controlled by peripheral sensory processes is decisively rejected—and with it all varieties of learning theory narrowly based on the reflex-arc concept. Second, the concepts of equipotentiality and "field dynamics" are examined and found wanting. As Hebb justly observes, Gestalt theories of visual organization have not avoided the difficulties presented by the anatomy of the visual cortex (p. 56). In consequence, he finds himself impelled to seek new conceptions of the central mechanisms governing behaviour more firmly based on the anatomy and physiology of the nervous system. Although the critical discussion is always informed and often acute, it is the constructive aspects of Hebb's work which merit the closest attention. These may accordingly be examined.

Hebb's positive contribution to psychological theory is given by two new conceptions, that of the *cell-assembly* and that of the *phase-sequence*. The cell-assembly is conceived as a specific and localized system of neurones firing in accordance with a sequence imposed by earlier patterns of excitation. It is held to comprise a large number of alternative pathways with equivalent functions and is thus endowed with a measure o f equipotentiality (p. 74). In the course of individual experience, cell-assemblies are held to become organized into larger functional units according to a principle of phase-sequence. This notion is never very adequately defined in general terms but its meaning becomes fairly clear in the course of the discussion of perception (pp. 79–106). In the visual perception of form, Hebb argues, the trend of recent evidence suggests that the essential mechanisms are in large part acquired. Recognition of form and pattern, he suggests, may be traced to the integration of a temporal sequence of oculo-motor adjustments (successive shifts of fixation) with the corresponding sequence of visual data. There is thus built up a specific sequence of cortical events each with its central motor component. When perception of a familiar visual form is demanded, it is supposed that a series of cell-assemblies fires off according to a particular phase-sequence determined by previous activity. Even when recognition becomes automatic, and takes place without overt eye-movement, its central basis must still be envisaged in terms of the sequential activity of a sensori-motor "schema." Hebb suggests further that manual and locomotor habits are to be analysed in the same way but no detailed illustrations are given. In general, it is argued that the concept of phase-sequence may provide a theoretical groundwork for dealing with all temporally organized nervous processes, not excluding those governing the train of thought (p. 100).

This bald statement of Hebb's theories in no sense does justice to the subtlety of the argument or to the wealth of pertinent data drawn from experimental and clinical sources. At the same time, the author is careful to point out that his notion of the cell-assembly differs little in principle from earlier conceptions. An unkind critic might even say that the whole argument is little more than the Jamesian theory of habit furbished up to meet the more exacting demands of present-day neurophysiology. But this would be unfair as well as unkind. Hebb's treatment, speculative though it is, does serve to throw emphasis upon important features of perceptual reaction signally neglected in current psychological theory. In particular, one may mention the importance attached to motor components in the general development of perception and the insistence upon a genetic approach to visual problems. These aspects of Hebb's work may well give rise to a much-needed change of attitude in many departments of experimental psychology.

The later chapters of this book are devoted for the most part to a discussion of learning, motivation and intelligence. In his treatment of motivation, especially, Hebb makes a most praiseworthy attempt to break away from the fixed ideas of contemporary American learning theory. It is not unfair to state that the latter envisages all modification as a quasi-conditioning process governed by those mythical phantasms—the drives. For Hebb, on the other hand, motivation is defined in terms of the organization, direction and persistence of phase-sequences (p. 181). That is to say, drive is an inherent property of the neural system upon which execution of a habit or skill depends. Although this notion needs working out in much greater detail, it is a notable advance in theoretical approach. There is also a sensible, if less original, treatment of learning in its comparative aspects and a good discussion of instinct. Hebb takes the view that instinct should not be regarded as fundamentally distinct from what is usually called intelligence. On the contrary, ". . . it is intelligence, or insight, that is innately limited in variety" (p. 169).

The book concludes with an entertaining discussion of emotion and a brief excursus into psychopathology (which includes two clinical histories of spontaneous neurosis in chimpanzees). There are also some shrewd and timely comments on intellectual deterioration and its problematical relations to the site and extent of cerebral lesion. Throughout, the emphasis is physiological but the author is refreshingly free from the limitations of doctrinaire behaviourism. He is interested in ordinary experience and ordinary conduct and is in search of the nervous mechanisms by which they are sustained and controlled. If he has failed to find them, at least he is steering in the right direction.

It is clear from this book that its author has made a sensitive and intelligent response to the major intellectual currents of his time. Although he clearly pins his ultimate faith on neurophysiology, he has learnt much from clinical neurology and neurosurgery, experimental and comparative psychology, and shrewd observations of animals and man in daily life. The only intellectual influence which has completely passed him by is that of Freud. In many, of course, this would be an advantage but in Hebb's case it is something of a limitation. His treatment of emotion, and indeed psychopathology in general, would assuredly gain from a determined attempt to come to grips with the Freudian position. Apart from this qualification (which may well be personal to the present reviewer), there is little in the book to which exception might be taken. One is struck, it is true, by a certain naïveté and by a tendency at times for enthusiasm to outrun discretion. This is most clearly shown in the overwhelming importance attached to von Senden's study of the restoration of vision in operated cases of congenital cataract. Much of the material assembled by von Senden is generally regarded as somewhat suspect and unsuitable as a basis for broad theoretical generalizations. One may comment, too, that the organization of the book is somewhat slap-dash and at times the argument becomes extremely difficult to follow. This may deter readers accustomed to the ordered "phase-sequence" of a scientific text. None the less, these are faults inseparable from constructive endeavour and merit the reader's indulgence. Hebb's book reflects a most creditable attempt to subdue the nervous system conceived as the instrument of behaviour, and to state points of view which at all events do not openly conflict with the facts disclosed by modern neurophysiology. In the present unhappy state of physiological psychology this is no mean achievement.

O. L. Z.

SOME RECENT PUBLICATIONS BY MEMBERS OF THE EXPERIMENTAL PSYCHOLOGY GROUP

The Framework of Human Behaviour. By J. M. Blackburn. London: Kegan Paul, Trench, Trubner & Co., Ltd., 1947. Pp. viii + 158. 12s. 6d. net.

Dimensions of Personality : a Record of Research. By H. J. Eysenck. London: Kegan Paul, Trench, Trubner & Co., Ltd., 1947. Pp. xl + 308. 25s. net.

Pilot Error : some Laboratory Experiments. By D. Russell Davis. Air Ministry (A.P.3139A). London: His Majesty's Stationery Office, 1948. Pp. 39. 9d. net.

Researches on the Measurement of Human Performance. By N. H. Mackworth. Medical Research Council Special Report Series No. 268. London: His Majesty's Stationery Office, 1950. Pp. 156. 4s. od. net.